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METAPHYSICAL INTERPRETATIONS OF SCIENCE

PART II*

3 *The Role of Metaphysics in the Period of Galileo and Newton*

In the conflict between the Roman Church and the advocates of the Copernican system, the Church did not deny that the heliocentric system was useful in ' saving the appearances ' or, as Comte would have said, ' in co-ordinating the observed facts.' But the Church strictly refused to admit that this new system was ' true ' in the sense that it was ' intrinsically intelligible ' or even plausible. This means that, in our terminology, the Church refused to admit the ' metaphysical truth ' of the doctrine of the mobility of the earth. In a certain respect the Church advocated a ' positivistic ' viewpoint and this explains why a man like the great French historian and philosopher of science, Pierre Duhem, could be a staunch advocate of ' positivism in science ' and a defender of the Roman attitude in the Copernican conflict. The Church admitted that this heliocentric doctrine could be ' mathematically true,' but maintained officially in its decree of 1616 that it was ' philosophically false.' This dual conception of truth corresponds exactly to the two criteria of truth which we find in Thomas Aquinas and which we discussed in Section 2.

We can understand this double approach well if we consider, on one hand, the censure which the theological ' Qualifiers ' reported in 1616 to Cardinal Bellarmine, the leading figure in the Holy Office, and, on the other hand, a private letter which the same Bellarmine had written in 1615 to a friend of Galileo. He wrote :

> I think that you and Galileo would act more prudently if you presented your opinion as a hypothesis and not as an absolute truth.

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> To assert that the earth is really moving is a very dangerous thing, because it would irritate the philosophers and theologians. . . . To prove that the hypothesis of the immobility of the sun and the moving earth saves the appearances is not at all the same thing as to demonstrate the reality of the movement of the earth. I believe that one can prove the first point, but I doubt strongly whether one can prove the second point and in the case of doubt one must not abandon the sense of the Holy Bible in which it has been interpreted by the holy Fathers.[1]

The decree of the Holy Office based on the report of the ' Qualifiers ' declared bluntly that the doctrine of the movement of the earth was ' philosophically absurd.' The word ' absurd ' meant that a statement was contradictory to the accepted Aristotelian philosophy. The evidence of the validity of that philosophy in the Thomistic as in other forms, was not based upon the findings of science but upon ' common-sense judgments ' which had their basis in the experience of everyday life which could not be disrupted by any advance in science, in the modern sense of this word.

This character of Thomistic philosophy is explicitly upheld by the contemporary advocates of Thomism. We may quote as an example M. J. Adler who is one of the most prominent defenders of Thomism in the U.S.A. In a small book, *St Thomas and the Gentiles*[2] (he means by ' Gentiles ' the disbelievers in metaphysics, the ' positivists,' as St Thomas meant by ' Gentiles ' the disbelievers in Christianity), Adler writes : ' Let me formulate the position which all Gentiles, however diverse in other aspects, convene in denying. It is that philosophy is a body of knowledge, not obtained by any of the specialised methods of empirical research and consisting of truths which are not dependent for their validity upon the findings of science.' It is obvious that what is denied by the ' Gentiles ' is exactly what the Thomists mean by ' metaphysics.' In this body of knowledge the findings of science are replaced by the principles of common sense which are independent of the advance in science. This can be shown easily from the writings of any contemporary Thomist. We may quote the most successful propagandist and populariser of Thomism in the U.S.A., Rev F. J. Sheen, S. J. He writes in his book, *Philosophy of Science*[3] : ' In a hundred years from now some philosopher of the Great Tradition will apply the immortal principles of common sense to judge the latest lyricism of a Barnes or a Whitehead of that future day.' Very explicitly this author stresses the immortality of the commonsense principles in contrast to the changing doctrines of science. He writes sarcastically about the philosophers who take their cue from contemporary science : ' To marry the spirit of any age means to be a widow in the next.'

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[1] Published by D. Berti, *Copernico e le vicende del sistema Copernicano in Italia*, Roma, 1876, p. 125

[2] M. J. Adler, *St Thomas and the Gentiles*, Milwaukee, 1938

[3] F. J. Sheen, *Philosophy of Science*, Milwaukee, 1934

This role of common sense as critique of science becomes even more conspicuous if we direct our attention now to Francis Bacon. In many respects, he dropped the belief in the Aristotelian and Scholastic philosophy ; he is credited with the foundation of an empirical philosophy, essentially different from the medieval one. But if we read his distinction between ' astronomy ' and ' philosophy ' we find that it does not vary much from the distinction defined by St Thomas. Bacon writes :

> Let it then be arranged, if you will, between philosophy and astronomy, as by a convenient and legitimate compact, that astronomy shall prefer those hypotheses which are most suitable for compendious calculation, philosophy those which approach nearest the truth of nature ; and that the hypothesis of astronomy shall not prejudice the truth of the thing, while the decisions of philosophy shall be such as are explicable on the phenomena of astronomy.[1]

We note here precisely the Thomistic and Aristotelian distinction between what is ' truth of nature ' and what is ' suitable for calculation.' We can understand better what Bacon meant by ' truth of nature ' if we investigate his attitude towards the Copernican system. We shall see clearly that by ' true ' in contrast with ' suitable for calculation ' he meant such general principles as can be understood by their analogy to statements of our everyday experience, or, in other words, to commonsense statements. Bacon writes :

> In favour of the earth (as immovable centre) we have the evidence of sight and inveterate opinion. . . . In favour of the sun, on the other hand, we have the consideration that that body which has the chief office in the system should occupy that place from which it may best act on the whole system and communicate its influence. And since the sun is that which seems most to vivify the world by imparting

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[1] Francis Bacon, *Thema Coeli* (written probably 1612). All the quotations are taken from *The Philosophical Works of Fr. Bacon*, edited by Ellis and Spedding, London, 1857.

> heat and light, it appears altogether right and in order that it should be placed in the middle of the world.[1]

All this argument in favour of the sun sounds as if it was an argument concerning where to place a guest of honour at a dinner table. It is taken from analogies with situations in everyday life. Of course, one can consider analogies of different types which may lead to very different preferences. As a matter of fact, Bacon eventually rejects the Copernican system because it violates the requirements of common sense. He writes :

> The separation of the sun from the planets with which he has so many affections in common is likewise a harsh step ; and the introduction of so many immovable bodies into nature, as when he (Copernicus) makes the sun and the stars immovable, the bodies which are peculiarly lucid and radiant, and his making the moon adhere to the earth in a sort of epicycle, and some other things he assumes, are proceedings which mark a man who thinks nothing of introducing fictions of any kind into nature provided his calculations turn out well.

It is interesting to note how Bacon, in the last sentence, accuses Copernicus of believing in science in the modern sense and of neglecting the analogy with commonsense statements. We shall see soon that a similar criticism was raised against Newton by philosophers who continued in a certain way the ' Great Tradition ' of Scholasticism, although they appeared to advance very different philosophical systems. When the ' idealist ' Berkeley and the ' empiricist ' Bacon attempted to draw the line between ' science ' and ' philosophy,' they did it in the old Scholastic way.

It is well known, however, that Newton understood practically by an ' advance in science ' an advance in ' saving the appearances ' and in ' better methods of calculation.' Neither St Thomas nor Francis Bacon would have regarded such achievements as findings of ' truth.' In all cases where Newton achieved his great and unique discoveries he built up science in this ' positivistic ' sense. When he came to a point where it was not possible to achieve an advance in science he looked for direct analogies to everyday experience. The most remarkable points in this line are presented in two Scholia of his *Principia*. In the first one Newton[2] recognises that the speed of the uniform motion of a system cannot be determined by any mechanical experiment. To give meaning to this speed of a body he introduces the expression ' absolute space ' which is formed as an analogy to an empirical space. He knew well that there is no possibility of ' operational definition.' In order to make the analogy more substantial Newton describes the absolute space as the ' sensorium of God.' In this way he introduces an analogy in the conventional theological sense ; he speaks of God as of a being with sense organs.

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[1] Francis Bacon, *Descriptio Globi intellectualis* (written probably 1612)

[2] I. Newton, *Mathematical Principles of Natural Philosophy*. Edited by F. Cajori, Berkeley (Cal.), 1946, Scholium following Definition VIII, p. 79

In a Scholium to the third book [1] Newton points out that from his theory it cannot be shown that all planets move in the same plane and in the same sense. He says : ' It is not to be conceived that mere mechanical causes could give birth to so many regular motions. . . . This most beautiful system could only proceed from the counsel and dominion of an intelligent and powerful being. . . .' Newton is fully aware that by this statement he merely describes an analogy between the universe and products of human skill. He states : ' . . . all our notions of God are taken from the ways of mankind by a certain similitude, which, though not perfect, has some likeness, however.' This means, bluntly speaking, that he interprets the motions in the planetary system by comparing them with the observable products of human intelligence.

We see here that in those ' marginal cases ' where no advance in physical science is immediately possible, Newton replaces derivations from abstract scientific principles like the ' Laws of Motion ' by a derivation from an analogy between the physical phenomena and the products of human intelligence which we know from our everyday experience.

It is noteworthy that in cases where Newton and his followers restricted themselves to justifying principles of science by their practical and observable results, they were attacked by contemporary philosophers. The most remarkable case is Berkeley's criticism against Newton's use of the calculus of fluxions. We know well that at that time the logical foundations of this calculus (which we call today differential and integral calculus) were not established satisfactorily. Newton's mathematical followers justified this calculus by its brilliant and practical results, particularly in the theory of motion. However, Berkeley, in his book, *The Analyst*, points out that the justification of a principle by its consequences does not constitute a criterion of truth.

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[1] *Mathematical Principles of Natural Philosophy*, Third Book, General Scholium, p. 544

What is needed is the ' evidence of the principle.' This is completely in line with Aristotle and St Thomas, according to whom the justification by consequences provides only an inferior type of knowledge which never leads unambiguously to truth. Berkeley puts the rhetorical question : Whether there can be science of the conclusion when there is no evidence of the principle ? And whether a man can have evidence of the principles without understanding them ? And therefore whether the mathematicians of the present age act like men of science in taking so much more pains to apply their principles than to understand them ?[1]

Incidentally, the last sentence could be used as a fair description of quite a few contemporary authors in quantum theory. Generally speaking, however, it means again an emphasis on the ' evidence of the principles ' as the higher degree of knowledge in contrast to the agreement of the consequences with experience which is stressed by modern science.

Such an ' evidence of the principles ' which is not based upon their observable consequences can consist only in a metaphysical interpretation which means, ultimately, the pointing out of some analogy with commonsense statements. Berkeley's objections can also serve as a good basis for the conjecture that such an interpretation is mostly advanced for a certain practical purpose, for bolstering up some ethical or religious doctrine. Berkeley says bluntly that the lack of evidence for the principles of mathematical science justifies some traditional religious doctrines which are recommended by their desirable effects on human behaviour, although the principles from which they are deduced may not be very evident. Berkeley asks in his *Analyst* (Qu. 60) : ' whether it be not a juster way of reasoning to recommend points of faith from their effects than to demonstrate mathematical principles by their conclusions ? ' The editor[2] of Berkeley's collected works characterises *The Analyst* as follows : ' It seeks to demonstrate to free-thinking mathematicians that while they accuse Christians of making large and ridiculous assumptions in religion they make assumptions of an equal sort in the exactest of the sciences. This Berkeley does by criticising Newton's doctrine of fluxions.'

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[1] G. Berkeley, *The Analyst, or a discourse addressed to an infidel mathematician wherein it is examined whether the objects, principles and inferences of the modern analysis are more distinctly conceived, or more evidently deduced, than religious mysteries and points of faith,* London, 1734, Qu. 36.

[2] *The Works of George Berkeley,* edited by G. Sampson, London, 1898, p. 3

4 *Science and Common Sense*

We noticed in the historical retrospect of Sections 1, 2 and 3 that the metaphysical interpretations of science had been closely connected with interpretations of science by means of commonsense statements. We are now going to look directly into the relation between scientific and commonsense statements. As we mentioned in Section 1, all the results of observations and experiments which form the factual basis of science can be described in the language of our everyday life, or, in other words, by commonsense statements. Professor Dingle says : ' The very practice of science itself is a commonsense undertaking.'[1] But when we build up the theories and principles from which the observable facts are to be derived in a convenient and logical way, we have to formulate these principles in a more abstract language which contains words and symbols which are very far from commonsense language. Professor Dingle writes : ' The undying glory of Galileo's contribution to thought is that, though only half consciously, he discarded the everyday, commonsense world as a philosophical necessity.'[2] As a matter of fact, the advance in science has consisted greatly in the replacement of the commonsense world by a world of abstract symbols.

That commonsense world consisted of matter and mind. We use both these words in the sense we have learned in our childhood, from our grade school and Sunday-school teachers. These words are fairly precise if we use them in the description of our commonsense experience ; they do not lead to any misunderstanding in this domain. For example, even an uneducated man can distinguish precisely between ' a fight with material and a fight with mental or spiritual weapons.' If I throw out a man with my hands, I make use of a ' material weapon,' but if I speak to a crowd as an advocate of ' democracy ' or ' communism ' and convince them by my words to throw this man out, I use ' mental weapons.' This distinction is quite familiar and clear to everyone who has passed through an elementary school. However, if I leave the commonsense level and want to use this distinction in a scientific discussion, it becomes much less clear, and fizzles away. I could describe my speech and its results by saying that I produced sound waves which made electromagnetic fields in the brain, which, in turn, acted upon the electrons and nuclei in the body of the man who was eventually thrown out. Then it is not obvious whether I really used a ' mental ' or a ' material ' weapon. The distinction cannot easily be made in the language of science.

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[1] H. Dingle, ' The Nature of Scientific Philosophy,' *Proc. of the Royal Soc. of Edinburgh*, 1949, **62**, part iv, 409

[2] Ibid. p. 403

In the domain of everyday experience we mean by ' matter ' merely what obeys the elementary laws of motion and by ' mind ' what obeys the most elementary and familiar laws of psychology. Therefore, the words ' mind ' and ' matter ' are legitimate words of the commonsense language but not of the scientific language.[1] Professor Dingle made a similar remark about the distinction between physics and chemistry. If we speak about directly observable facts in commonsense language (particularly in applied chemistry), the distinction is easily understood and is even practical, whereas if we speak on the level of modern atomic physics and nuclear physics the distinction loses any meaning. Chemistry is today a commonsense term but not a scientific term. Professor Dingle says : ' The truth is that chemistry indeed has no place in the strict scientific scheme. . . . The part played by chemistry in the growth of science has been a pragmatical, heuristic one.' [2] The same thing can be said about the place of the words ' matter ' and ' mind ' in the history of science.

If we look into the evolution of science in our twentieth century we notice that the terms which are used in the general principles have become more and more remote from the terms which are used in commonsense language. We have merely to compare Einstein's theory of gravitation with Newton's principles. Einstein's field equations contain as symbols the tensor of the gravitational field. It is a long way which leads from this tensor to the measurements of distance and time by which the values of this tensor can be actually tested. The ' force ' in the Newtonian laws has appeared to be much nearer to actual sense observations, although we understand today that this was largely due to an illusion. This increasing distance between the symbols of science and direct sense observations is very precisely described in Einstein's ' Herbert Spencer Lecture ' given at Oxford in 1933. Einstein speaks about the ' ever widening gap between the basic concepts and laws on the one side and the consequences to be correlated with our experience on the other, a gap which widens progressively with the developing unification of the logical structure, that is with the reduction of the logically independent elements required for the basis of the whole system. . . .'[1] The more the principles of science are improved, the more they become different from commonsense statements.[2]

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[1] P. Frank, *Foundations of Physics*, Chicago, 1947

[2] H. Dingle, *loc. cit.*, p. 409

Our observations and experiments, however, have been described invariably in commonsense language, notwithstanding all changes in the principles. Hence, science has become accustomed more and more to the use of different languages in the same picture of the universe, and it has become an important task of the scientist to fit these different languages into one coherent system. Professor Dingle says rightly : 'If I emphasise the necessity of freeing scientific philosophy from the intrusion of commonsense conceptions, it is not in order to depreciate common sense but because the greater danger today lies in their confusion.'[3]

We notice this confusion strongly in many presentations of quantum theory, where commonsense terms and symbols of the theory are mixed up in a way which gives the layman the impression that there is something 'irrational' and 'illogical' in modern physics in contrast to the 'rational' character of 'Victorian' physics.

The gap between the principles of science and commonsense statements has produced a certain feeling of frustration among those scientists and laymen who have longed for an 'understanding' of science. The twentieth century principles of science cannot be connected directly with rules of human behaviour as Aristotle's physics had been connected with the rule that everything in the universe 'has a natural place where it belongs.' For this reason, there has been a longing for a short cut between scientific principles and commonsense statements. To achieve such a short cut has been the conscious or unconscious purpose of the metaphysical interpretations of science.

If we want to find out what the authors actually did who added to 'science' a 'metaphysical interpretation,' we meet the difficulty that these two types of intellectual activity have been carried out, for the most part, by men of very different backgrounds. They have used diverse vocabularies and there has been little satisfactory communication between them. Therefore, it seems advisable to direct our attention to such men as were working in both fields, science and metaphysics. These men must have felt the need to define the precise relation between both types of search for truth. We easily find such men in previous centuries, men like Descartes, Pascal or Leibniz, but there are few in the nineteenth and twentieth centuries. One can certainly find some hint on this issue in A. N. Whitehead.

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[1] Reprinted in the collection of essays : A. Einstein, *Out of my Later Years*, New York, 1950

[2] P. Frank, 'Einstein's Philosophy of Science,' *Review of Modern Physics*, 1949, **21**

[3] H. Dingle, *loc. cit.*, p. 403

I found the most precise distinction in the work of Charles Sanders Peirce. He was equally devoted to science and to metaphysics and there has been quite a dispute as to whether he was essentially a positivist or a metaphysician. However this may be, he formulated first, in 1878,[1] what has been called since the ' pragmatical criterion of meaning ' and did it in a form which is compatible with the criteria later formulated by Wittgenstein, Carnap and the Logical Empiricists. The views of these groups are much more closely related to C. S. Peirce than to the American pragmatists of the school of William James.

We may expect, therefore, that the way in which C. S. Peirce defined the distinction between ' science ' and ' metaphysics ' will be particularly valuable for our purpose. C. S. Peirce outlined a classification of human knowledge in which he specifically attempted to draw a line between ' science ' and ' metaphysics,' or, in his terminology, between ' special science ' and ' philosophy.' Speaking of different classes of knowledge he says :

> Class II is philosophy which deals with positive truth, yet contents itself with observations such as come within the range of every man's normal experience. . . .
> Class III the special sciences depend upon special observation, some assistance given to the senses, either instrumental or given by training, together with unusual diligence. . . .[2]

In these lines Peirce stated very clearly that ' philosophy ' is built upon everyday life experience and not upon scientific research. Therefore, it is independent of the changing theories of science. ' Philosophy,' as Peirce points out, does not make use of special training in science nor even of unusual diligence. All this amounts to tying up ' philosophy ' with the conceptual schemes that we need to deal with the experience of our daily life or, in other words, with our commonsense world.

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[1] C. S. Peirce in the paper, ' How to make our ideas clear,' *Popular Science Monthly*, 1878, **12.** Reprinted in the *Collected Papers*

[2] C. S. Peirce, *Collected Papers*, Harvard Press, Cambridge (Mass.), 1931, I

Peirce stresses the point that ‘ science ’ departs much more from the commonsense discourse than ‘ philosophy,’ but he adhered to the empirical criterion of meaning in its strictest form. He says : ‘ A phrase between the assertion and the denial of which there can in no sense be any sensible difference is gibberish.’ [1]

No ‘ logical positivist ’ has ever formulated this criterion in a more ‘ aggressive ’ way. We should give, therefore, particular attention to Peirce’s views on metaphysics as distinct from science.

5 *The Short Cut from Science to Common Sense*

To understand precisely what Peirce actually meant by connecting ‘ philosophy ’ with ‘ everyday experience ’ and ‘ science ’ with ‘ unusual diligence,’ we have to examine the way in which he applied this distinction to special cases. In his day, the question was discussed frequently whether the properties of matter can be derived from the hypothesis of an atomistic or continuous structure. The physicist would attack such a question by deriving from each hypothesis, by mathematical conclusions, observable facts which can be checked by experiments that require a more or less elaborate technique. However, Peirce writes : ‘ A question at this moment under consideration by the physicists is whether matter consists ultimately of minute solids or whether it consists of vortices of an ultimate fluid. The question must evidently depend upon what we ought to conclude from everyday unspecialised observation.’

This means that the ‘ minute solids ’ and the ‘ vortices in an ultimate fluid ’ are not abstract concepts of a physical theory. If this were the case one would have to check this theory by mathematical and experimental techniques and not by ‘ everyday unspecialised observation.’ Peirce means, obviously, that these ‘ solids ’ and ‘ vortices ’ and these ‘ ultimate fluids ’ are objects of our everyday experience which are to serve as pictures of the unknown physical reality. They are not to be checked by scientific methods but by examining which picture is more appealing to our common sense, the ‘ minute solids ’ or the ‘ vortices in a continuous fluid.’ Both these objects are not concepts of a physical theory but commonsense objects which serve as analogies in a ‘ metaphysical interpretation ’ of physics.

In this way a relation between common sense, science and metaphysics is established which plays a part whenever a scientific theory is interpreted 'philosophically.' The precise character of this relation was described very lucidly by the French philosopher, Edouard Le Roy. This author is known to a great many scientists because he is frequently mentioned in H. Poincaré's writings.[1] Le Roy was an advocate of Scholastic Metaphysics and he made great efforts to prove that this metaphysics is justified by H. Poincaré's very critical philosophy of science. To prove his point, Le Roy had to define precisely the place of metaphysics within the frame of scientific thinking. He found this place in the gap between science and common sense which had been produced by the increasingly abstract formulation of the principles of science. Le Roy says : 'Science departs from common sense and does not join it in its development as science proper. Thus science by itself does not close the cycle of knowledge and does not realise the unity of knowledge. Science, in turn, needs, therefore, a "prolongation" and this will be "philosophy" . . .' In the same way, science itself is a prolongation of common sense.[2]

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[1] *Collected Papers*, **6**, Ch. 5, § 3

This scheme characterises adequately the place of the 'metaphysical interpretations of science.' There are two paths which connect the principles of science with commonsense statements. The scientific path is a long one. It leads by mathematical conclusions to observational facts which can be checked by physical experiments and which can be described in commonsense language. The second path, the metaphysical one, is a short cut. It gives, without mathematical and experimental technique, a picture, an analogy of the scientific principles. This picture can again be described in commonsense language. These two paths together, the long and the short one, form a cycle. As a simple illustration we can use the example from which we started in Section I. From the principle of conservation of energy we can derive, by using the equations of thermodynamics, relations between measurable quantities (temperature, heat capacity, mass, velocity, etc.) and can check these relations by actual measurements which can be described in commonsense statements as, for example, the statement that the top of the mercury column in a thermometer coincides with the mark '100' of a scale. This is the long, scientific path between scientific principles and commonsense statements.

We can also bring about the connection by a 'short cut.' We can interpret ' energy ' as a ' thing.' Our commonsense experience teaches us that ' no thing can disappear.' Kinetic energy and heat energy are two ' things.' Neither of them can disappear, but we can regard both things as one and the same thing in two different ' states.' Then, kinetic energy cannot disappear, but can be converted into heat energy. In the same way, we could analyse every physical theory and its metaphysical interpretations. In a great many, perhaps in all, cases, the metaphysical interpretation is actually a short cut which replaces the long and difficult path of science by a presentation which makes use of only the most elementary part of science, the description of our everyday experience.

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[1] H. Poincaré, *La Valeur de la Science*, Paris, 1905

[2] E. Le Roy, 'Science et Philosophie,' *Revue de Métaphysique et de Morale*, 1899, 7, 375ff.

This rather ' robust ' role of metaphysics and the ' subtle ' role of science are clearly distinguished in the classification of the sciences suggested by the French philosopher, Edmond Goblot. He says : ' Physicists and metaphysicians arrive at principles of physics which are very different from each other. . . . Physicists start from concepts by means of which one can construct all other concepts, they arrive at these principles at the end of long research work and of profound analysis. The metaphysicians start from the simplest beings and the most palpable facts, taking their first elements from most common experience '.[1]

Goblot characterises very aptly the paradoxical situation in the domain where science and metaphysics meet. In contrast to the view which has been commonly held about the nature of metaphysics he writes : ' It is theoretical physics which is really abstract ; it is metaphysics which allows a piece of empiricism. It may seem unexpected that metaphysics, which flatters itself to proceed *a priori*, deserves this reproach of empiricism while physics, an experimental science, turns out to be perfectly rational.'

Goblot recognises very well that the seeming self-evidence of metaphysical statements is not based upon their rationality but upon commonsense experience which is more ' down to earth ' than scientific experience. He says : ' Despite its rationalist and apriorist tendencies, metaphysics in its search for the real cannot attain it without an appeal, at least a disguised one, to experience, since only experience can teach us what is.'

By using this terminology we can express, in a succinct way, the attitude towards metaphysics which has been held in this paper.

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[1] E. Goblot, *Le Système des Sciences*, Paris, 1922

We may say that the major part of what is taught under the name of ' metaphysics ' consists of commonsense statements ' disguised ' as results of pure rational thinking.

6 *Conclusion and Outlook*

If we regard metaphysics, as is suggested in this paper, as a short cut between science and common sense, we acquire a new perspective of human thought in the past and the present. I shall merely outline briefly two applications, the first to the history of medieval philosophy and the second to the influence of twentieth century science on the intellectual climate of this period.

It has been stated repeatedly that the central concept in Thomistic metaphysics is the concept of ' analogy.' By the use of this concept, ' metaphysics ' is sharply distinguished from ' science.' Phelan says in his book on St Thomas : ' The importance of analogy in the philosophy of St Thomas literally cannot be overestimated '.[1] The leader of contemporary Neo-Thomism, J. Maritain, remarks : ' This universe in which metaphysics issues . . . is not conatural to our powers of knowledge. It is only intelligible to us by analogy.' [2]

This concept of ' analogy ' has been discussed little outside the circle of specialists in Thomistic philosophy. I submit that ' analogy ' is the central concept in every kind of metaphysics and that what the Thomists mean by ' analogy ' is about the same as the ' direct commonsense interpretation of abstract principles,' the ' short cut from science to common sense ' about which we have spoken in this paper. I shall drop this point here and resume it on some other occasion.

I would like to conclude my paper with a few remarks on the current philosophical interpretations of twentieth century physics. Every reader of magazines and even daily papers knows that the conversion of mass into energy has been used to refute metaphysical materialism and to bolster up metaphysical idealism. Everyone knows also that quantum theory has been employed to refute the doctrine of ' iron causality ' and to bolster up the metaphysical doctrine of ' free-will.' There have been passionate debates about these interpretations among scientists, among philosophers, between scientists and philosophers, between philosophers and clergymen, and even between political parties. Since ' materialism ' and ' idealism ' have

[1] G. B. Phelan, *St Thomas and Analogy*, Milwaukee, 1941

[2] J. Maritain, *The Degrees of Knowledge*, New York, 1938

become political slogans we must not wonder that these debates have produced more heat than light.

If we look at these debates from the viewpoint of this paper we shall recognise that all these implications of modern physics (idealism, indeterminism, etc.) do not belong to science but to the ' metaphysical interpretations of science.' They cannot be checked by mathematical calculations or physical experiments, but they are all more or less short cuts which connect the principles of, say, quantum theory, directly with the facts of everyday life experience. It seems to me that on this basis all these debates could be conducted with much greater prospect of a satisfactory result, but we shall elaborate this suggestion on some other occasion.

PHILIPP FRANK

ON THE FOUNDATIONS OF DYNAMICS

1 *The Doctrine of Inertia*

Of the three basic mathematical disciplines, Euclidean geometry, elementary arithmetic and classical dynamics, modern criticism of the first began in the eighteenth century, of the second in the late nineteenth, and criticism of the third entered on an entirely new era with the vast extensions in knowledge dating from the beginning of the present century. Just as the generalisations of geometry and arithmetic have gone hand in hand with the development of a more profound analysis of the traditional geometry and arithmetic, so the dazzling successes of the new forms, and generalisations, of classical dynamics have not diverted attention from the problem of analysing the Newtonian laws of motion, probing their nature and assessing their epistemological status. Over three hundred years have elapsed since Galileo stated that his purpose in formulating his dynamics was to set forth ' a very new science dealing with a very ancient subject ', and the most recent developments reveal that, *mutatis mutandis*, not only is discussion of the foundations of this science almost as controversial today as it was in the early seventeenth century, but also it is still fruitful of new ideas.

Recently, we have been reminded by Professor Butterfield[1] that ' Of all the intellectual hurdles which the human mind has been faced with and has overcome in the last fifteen hundred years, the one which seems to have been the most amazing in character and the most stupendous in the scope of its consequence is the one relating to the problem of motion. . . . It was supremely difficult to escape from the Aristotelian doctrine by merely observing things more closely, especially if you had already started off on the left foot and were hampered beforehand with the whole system of interlocking Aristotelian ideas. In fact, the modern law of inertia is not the thing you would discover by mere photographic methods of observation—it required a different kind of thinking-cap, a transposition in the mind of the scientist himself: for we do not actually see ordinary objects continuing their rectilinear motion in that kind of empty space which Aristotle said could not exist, and sailing away to that infinity which

[1] H. Butterfield, *The Origins of Modern Science*, London, 1949, p. 3

also he said could not possibly exist; and we do not in real life have perfectly smooth balls moving on perfectly smooth horizontal planes —the trick lay in the fact that it occurred to Galileo to imagine these.'

Elsewhere, the same author [1] has remarked, ' It has been suggested that the real secret of the new attitude to motion lies in the interest which had been awakened in Archimedes, and which encouraged a habit of mathematising a problem—seeing a purely geometrical body moving indefinitely in Euclidean space, without air-resistance and without the complication of the effect of gravity. People talk glibly about the experimental method in the seventeenth century, but subtle changes like these are hardly to be accounted for by closer observation —they entail a different attitude, so to speak, in the observer.'

In the early disputes about the foundations of natural philosophy and dynamics we find time and again that the crucial arguments turned, as they still do today, on the distinction between mathematical idealism and the evidence of the senses. This distinction was reflected in the famous doctrine of primary and secondary qualities: Galileo's primary qualities, length, time, etc., were essentially mathematical. His scholastic opponents, on the other hand, were continually appealing to simple and plain observations of actual concrete phenomena; they were, in short, the positivists of their day.[2] The founders of modern physical science, however, were far more akin to the much criticised Jeans, Eddington and Milne of our own time. One of the greatest of the forerunners of Copernicus and Galileo, Cardinal Nicholas of Cusa (1401-1464), enunciated the extreme doctrine that ' mathematics is the basis of all knowledge ', a slogan which has continually been re-echoed, notably by Kant in his exaggerated statement [3] that ' a doctrine of nature can only contain so much science proper as there is in it of applied mathematics '. Of Copernicus, we find that the high-priest of modern empiricism, Francis Bacon, complained that ' Copernicus was a man who did not care what fictions he introduced into nature provided his calculations answer ', and it is well known that Kepler, like Galileo, had a highly mathematical conception of nature. Their outlook was both clearer and cruder than ours : whereas for us mathematics provides a highly convenient way of looking at things, for them nature was ' really mathematical ', a belief which in recent years has become uncommon, though it has been held by some writers, for example, Jeans.

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[1] H. Butterfield, *The Listener*, 15th July, 1948, p. 96

[2] It is perhaps not without significance that the recent association of positivism and the logical syntax of language finds its parallel in the Aristotelian philosophies of the late Middle Ages.

[3] I. Kant, *Prolegomena and Metaphysical Foundations of Natural Science*, translated by E. Belfort Bax, London, 1883, p. 141

The mathematical conception of nature was both supplemented, and in a sense even made possible, by the doctrine of the uniformity of nature. In the Middle Ages it had been taken for granted that all motions in the celestial realm were based on motion in a circle, whereas in the terrestrial realm the fundamental motion was rectilinear and directed downwards. Unlike motion in a straight line, circular motion could, of course, continue indefinitely in a finite universe with an outer boundary. Galileo, however, believed that the same laws of motion operated both in the celestial and in the terrestrial realms, and he was strongly attracted to the subject which we now call dynamics, because he thought of it partly as a *cosmological* science, believing that, if he could discover the laws of terrestrial motion, then he could apply them to motion in the heavens.

Galileo's three outstanding contributions to the development of classical dynamics were his laws governing the resolution and composition of independent motions, his law of falling bodies, and his implicit recognition of the principle of inertia. Discussing Galileo's investigation of falling bodies, a distinguished historian of science [1] has recently written ' In " experiments near a hundred times repeated ", Galileo found that the times agreed with the law, with no differences " worth mentioning ". His conclusion that the differences were not " worth mentioning " only shows how firmly he had made up his mind beforehand, for the rough conditions of the experiment would never have yielded an exact law. Actually the discrepancies were so great that a contemporary worker, Père Mersenne, could not reproduce the results described by Galileo, and even doubted that he had ever made the experiment.'

The same author points out that Galileo worked out his physics by thought, by correct reasoning and mathematics, not by induction from experiments. ' Galileo's writings abound with reference to the facts of experience, of direct observation. In this sense, Galileo built his science on a somewhat empirical basis. But he was in no sense such an empiricist as the nineteenth-century writers attempted to make him out. He was not a careful experimenter, though he was

[1] I. B. Cohen, *Scientific American*, 1949, 181, 45

a keen observer, and it is only the fallacy of writing history backwards that has made us visualise him as the patient investigator who only reluctantly drew conclusions after long tests. The latter type describes a much later type of scientific man. . . . Galileo's greatest contribution was the idea that mathematics was the language of motion, and that change was to be described mathematically. . . . The most important influence on Galileo's thinking undoubtedly was Archimedes, but whereas the latter had constructed a geometry of rest, Galileo built a geometry of motion.'

In his theory of projectiles, Galileo introduced, at least implicitly, the idea of inertia : the revolutionary concept that uniform motion in a straight line is physically equivalent to a state of rest. The law of inertia is intimately associated with the relativity principle in so far as it concerns uniform motion, a principle which can be traced back through Copernicus to Nicholas of Cusa. The explicit enunciation of this relativity principle and of the law of inertia is due to Newton. In the *Principia*, Corollary V to the Laws of Motion, the relativity principle is stated in the following form : ' The motions of bodies enclosed in a given space are the same relatively to each other whether that space is at rest or moving uniformly in a straight line without circular motion '. Both this principle and the law of inertia presuppose the concept of inertial frames (the term is a modern one). The law of inertia was enunciated by Newton in the form : ' Every body continues in its state of rest or of uniform motion in a straight line, except in so far as it may be compelled by impressed forces to change its state '. This law and the relativity principle of Corollary V comprise the doctrine of inertia.

Recent scholarship has revealed that many of the ideas associated with this doctrine were anticipated as early as the fourteenth century. Although the decisive step of raising the concepts of space and time to the level of fundamental categories was primarily due to Galileo, discussions on the nature of motion were eagerly pursued by the late medieval philosophers. Duns Scotus, for example, argued that motion was a disposition in a moving body, a quality distinct both from the body and from the space it occupied, and Jean Buridan, who accepted this theory, held that the continuance of motion in a projectile after leaving the projector was brought about by an ' impetus ' impressed on it which acted as the efficient cause of motion. The followers of William of Ockham, however, denied that movement was due to the real existence of some form in the moving body.

They argued that it was sufficient to regard the moving body as having at different instants different spatial relationships to some other body. In particular, the observation that a well-shaped ball keeps its motion on a polished surface for a considerable time had long been familiar, but its interpretation was a source of dispute. The more conservatively minded medieval schoolmen ascribed the phenomenon to the influence of the medium, but this view was criticised, notably by Nicholas of Cusa in his treatise *De Ludo Globi* (a kind of billiards), who explained this observation in terms of the ' impressed force ' communicated by the original blow. The Ockhamists and following them Galileo sought, however, to identify this suggested cause of the motion with the motion itself, which therefore came to be regarded as self-perpetuating. *Entia non sunt multiplicanda praeter necessitatem.*

2 *Critique of the Law of Inertia*

The new ideas concerning motion, which were formulated in the Scientific Revolution, acquired a predominantly mathematical character through the influence of Galileo and Newton, and in consequence they spread very slowly. Indeed, nearly three hundred years separated Nicholas of Cusa from Voltaire, who played a prominent part in introducing Newtonian ideas on the Continent, about as long as the time that has elapsed since the death of Galileo and the birth of Newton. The keystone of the new philosophy was the law of inertia. In examining the nature of this law we will follow historical precedent and first consider the view that the law is false, and even absurd.

I *The Law of Inertia is False.* On examining Newton's wording of the law, we immediately query the use of the word ' body '. As Karl Pearson [1] pointed out more than fifty years ago, ' the reader who is acquainted with treatises on dynamics will remember that one of the most difficult chapters is frequently entitled " Motion of a body under the action of no forces " '. Not only may such a body rotate about an axis, but, in general, the axis about which it spins may itself be continually changing its position. In point of fact, then, the ' state of rest, or of uniform motion in a straight line ' is *not* that which the physicist postulates to describe the motion of a *body* under no forces. It might be argued that the law refers to the centre of mass of a body; this interpretation, however, would depend on Newton's third law of motion concerning action and reaction, and the status of the latter and the definition of centre of mass have become somewhat obscure as the result of recent relativistic theories.[1] In any case, it is clear that Newton's first law is not a descriptive law applying to the behaviour of actual bodies. It applies to particles, but these are conceptual, for Newtonian dynamics with its presuppositions concerning precise location in space and time is not appropriate to the study of the actual fundamental particles occurring in Nature. Classical dynamics applies to vast aggregates of these, but the Newtonian particle is an abstraction from the aggregate.

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[1] K. Pearson, *The Grammar of Science*, London, 1937, p. 272

These objections to the traditional wording of the law of inertia are recent. Let us now consider briefly how the law appeared to philosophers in the early part of the eighteenth century. In the British Isles, Bishop Berkeley wrote an essay *De Motu* criticising in detail the Newtonian philosophy of motion, while on the Continent Leibniz similarly argued that Newton's law of inertia was philosophically false since, in his opinion, it was absurd to maintain that a moving body could keep its direction with respect to the empty space, for how could the empty space exert any action?

II *The Law of Inertia is Self-Evident.* By the end of the eighteenth century the 'new philosophy' of Galileo and Newton had been triumphantly vindicated in planetary astronomy by Euler, Lagrange, Laplace, Clairaut and others. Consequently, philosophers now claimed that the fundamental laws of Newtonian mechanics could be derived from pure reason. What for their predecessors had been a falsehood now became a platitude. For example, Kant[2] in his discussion of the metaphysical foundations of mechanics argued that matter is subject to no change except through motion, and that any change of motion must be due to some 'external' cause. In this attempted derivation of the law of inertia the whole force of the argument, which in fact is extremely obscure, was directed to proving that changes in the *motion* of a body necessarily result from *external* causes, whereas in the previous century it had appeared all-important to establish that the crucial phenomenon in dynamics is change of motion rather than change of position. This point seems to have been completely ignored by Kant. More generally, we can argue against the *plausibility* of the thesis that the law of inertia is self-evident in view

[1] M. H. L. Pryce, *Proc. Roy. Soc. A,* 1948, **195**, 62

[2] Kant, *op. cit.*

of the prolonged arguments which preceded and accompanied its original formulation.

Turning from philosophers to men of science, we find that they too came to adopt a not fundamentally dissimilar attitude. No theory could possibly be regarded as satisfactory if it failed to prove that the observed phenomena were derivable from Newton's laws. Indicative of the mental climate generated by nearly two centuries of the 'new philosophy' is Helmholtz's declaration[1] in 1847: 'The task of physical science is finally to reduce all phenomena of nature to forces of attraction and repulsion, the intensity of which is dependent only upon the mutual distance of material bodies. Only if this problem is solved are we sure that nature is conceivable.' Similarly, du Bois Reymond in his famous address, in 1872, *On the Limitations of Natural Science*, argued: 'The cognition of nature is the reduction of all changes in the physical world to the motions of atoms governed by forces independent of time. It is a fact of our psychological experience that our need for causal understanding is satisfied if we succeed in performing that reduction.'

In this spirit, the greatest mathematical physicist of the nineteenth century, Clerk Maxwell,[2] endeavoured to prove, in 1877, that the law of inertia was true *a priori*. After discussing the experimental arguments for the law, he suggested that our conviction of the truth of the law might be strengthened by considering the consequences of denying it. If, at a given instant, a body in motion were left to itself subject to no external force and its velocity did not remain constant, its instantaneous change of velocity would have a definite direction and magnitude, which he maintained would have to be the same whatever the time or place of the experiment. In the first place, he supposed the law to be that the velocity diminished at a certain rate which might be so slow that no experiments on moving bodies could detect the diminution of velocity in hundreds of years. The velocity referred to in such a hypothetical law could only be velocity referred to a point absolutely at rest, for if it were a relative velocity its direction as well as its magnitude would depend on the velocity of the point of reference. Consequently, if when referred to a certain point the body appeared to be moving northward with diminishing velocity, we would only have to refer it to another point moving northward with a uniform velocity greater than that of the body, to find that the body would then appear to be moving southward with increasing velocity. Clerk Maxwell therefore concluded that such a hypothetical law would be devoid of meaning, unless the possibility of defining absolute rest and absolute velocity was admitted into physics, and that the denial of Newton's law was ' in contradiction to the only system of consistent doctrine about space and time which the human mind has been able to form '.

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[1] H. L. F. Helmholtz, *On the Conservation of Energy*, 1847

[2] J. Clerk Maxwell, *Matter and Motion*, London, 1925, p. 28

Nevertheless, Clerk Maxwell evidently had some misgivings about his argument, for he supplemented his remarks with an elaborate footnote in which he claimed that his argument could be made ' more definite '. He pointed out that observation showed that the more *isolated* a body was from the influence of other bodies, the more nearly would its velocity remain constant with reference to an assignable frame of reference. A frame of space and time with respect to which this principle is strictly valid is called (after James Thomson) a frame of inertia. Given one frame of inertia, any other frame moving with uniform motion in a straight line with respect to it is also a frame of inertia. For local purposes a first approximation to a frame of inertia is one fixed with reference to the surrounding landscape ; but astronomers have to adopt a frame containing the axis of the earth's diurnal rotation, which in a further analysis has to be corrected for the very slow movement of the earth's axis that is revealed by the precession of the equinoxes, and so on. In Maxwell's view, such a frame of inertia represents in its practical essentials the Newtonian absolute space and time, being ' the simplest and most natural scheme of mapping an extension into which dynamical phenomena can be fitted '.

An obvious weakness running through Clerk Maxwell's argument is that *a priori* we could begin by substituting acceleration for velocity in his discussion. Larmor[1] made a similar criticism when he pointed out that, as Newton's concept of dynamical force depends on the rate at which the velocity of the body on which it acts is changing with the time, it might legitimately be queried why this particular measure should correspond to objective nature rather than one involving, for example, the velocity also, or the rate of change of the acceleration.

III *The Law of Inertia is Empirical.* Despite the various attempts to demonstrate the *a priori* nature of the law of inertia, it has been frequently maintained that the law is empirical. A recent supporter of this view is Philipp Frank,[1] who denies that the law can be derived from any self-evident principle and claims that ' obviously this law is empirical '. He argues that the only element in the law which appears to him to be at all self-evident is the implicit assertion that the straight line is the ' simplest ' curve, but he maintains that this too is an observational statement describing psychological facts rather than physical ones. In his view ' The law of inertia has an operational meaning only if the inertial system is described by a physical operation. As a first approximation we can say that S (an inertial frame) is identical with the system of the fixed stars. Since a particular physical object (the fixed stars) enters into this law, it cannot be a self-evident statement. It is even clear that the law of inertia, if given this operational meaning, cannot be exactly true.' Consequently, if we begin by postulating the purely empirical nature of the law, we end by reverting to the first view that strictly speaking the law is false.

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[1] J. Larmor, Appendix I, p. 137, to Clerk Maxwell, *op. cit.*

If the law of inertia were empirical, as Frank asserts, its origin would be purely inductive, but it could not depend on inductive generalisation in the ordinary sense because it would be based on extrapolation from actual cases to an empirically impossible case. Moreover, the principle of Aristotelian physics that motion passes of itself into rest has *prima facie* more observational evidence in its support than Galileo's principle of inertia. In Galileo's *Two New Sciences*, Simplicio, the spokesman of Aristotelian physics, is a stubborn observationalist. Galileo's ideal forms of motion, in his opinion, are all right mathematically, but there is nothing in Nature that corresponds to them.

IV *The Law of Inertia is* a Priori *and Empirical.* A compromise solution of the problem of the nature and status of the law of inertia has been suggested by the distinguished philosopher of science Emile Meyerson,[2] who argues that the law is neither *a priori* nor *a posteriori* because it is both at the same time. He suggests that ' Perhaps it would be wise to apply to statements of this category, intermediary between the *a priori* and the *a posteriori*, a special term. We should propose, for lack of a better one, the term *plausible*. Therefore, every proposition stipulating identity in time, every law of conservation, is plausible.' In support of this contention, Meyerson claims that the principle of

[1] P. Frank, ' Foundations of Physics ', *International Encyclopaedia of Unified Science*, Chicago, 1946, vol. 1, No. 7, p. 13

[2] E. Meyerson, *Identity and Reality*, London, 1930, p. 148

inertia demands that we conceive of velocity as a substance, and that we accept this strange notion because it serves to satisfy our belief in causality. Meyerson, of course, was a chemist!

V *The Law of Inertia is not the Unique First Axiom of Dynamics.* The possibilities which we have considered so far are concerned with the question of whether the law of inertia is *a priori* or *a posteriori*, and we have assumed implicitly the traditional view that the law is the unique first axiom of dynamics. A pioneer in the criticism of this assumption was the great German physicist Heinrich Hertz.[1] Convinced that no further experience would find anything to alter in the firm foundations of dynamics, he was nevertheless dissatisfied with the purely empirical approach because 'that which is derived from experience can again be annulled by experience'. He was led to consider what has come to be regarded by many as the key problem in the philosophy of science: the problem of the isomorphism of mind and nature. He maintained that throughout the history of natural philosophy there has been a fundamental principle guiding us in our construction of mental images or symbols of objects in the external world: the necessary consequences in thought (i.e. the logical consequences) of our images should be the images of the necessary consequences in nature (i.e. the empirical consequences) of the things pictured. Hertz emphasised that this is the only isomorphism which is essential for the development of physical science, and moreover, that we have no means of knowing whether any other isomorphism is possible.

More recently this point of view has been presented in a sharper perspective by Dirac[2] in his well-known statement that '*The only object of theoretical physics is to calculate results that can be compared with experiment*, and it is quite unnecessary that any satisfying description of the whole course of the phenomena should be given'. This attitude is, of course, in striking contrast to that of Galileo[3] who believed that 'Philosophy is written in that great book which ever lies before our eyes—I mean the universe. . . . This book is written in the mathematical language.' The modern view is indicative of a radical change in the climate of opinion in scientific thought, for we now regard our 'images' as sufficient rather than necessary. In principle, therefore, we no longer believe that the isomorphism is *unique*. Consequently, an element of *choice* has entered into our problem.

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[1] H. Hertz, *The Principles of Mechanics*, translated by D. E. Jones and J. T. Walley, London, 1899

[2] P. A. M. Dirac, *The Principles of Quantum Mechanics*, Oxford, 1930, p. 7

[3] G. Galileo, *Il Saggiatore*, question 6; *Opere*, Firenze, 1890, **6**, 232

Contrasting the logical obscurity and the empirical success of classical mechanics (in his day there were no limitations), Hertz complained that in expounding the foundations of mechanics in the traditional manner he felt tempted now and again to apologise and to hurry on as quickly as possible to examples which spoke for themselves. His objections related to the traditional form of the 'image' rather than to its content. He thought that this representation had not attained scientific completeness as 'it still fails to distinguish throughly and sharply between the elements in the image which arise from the necessities of thought, from experience and from arbitrary choice'.

VI *The Law of Inertia is a Convention.* As soon as an element of choice enters into our problem, we are naturally led to examine the possibility that the law of inertia is simply a convention. Actually, this idea appears to have been first contemplated more than seventy years ago by Thomson and Tait in their famous treatise *Natural Philosophy*. They suggested that the law could be stated in the form: 'The times during which any particular body, not compelled by force to alter the speeds of its motion, passes through equal spaces are equal'. Worded thus the law expresses our convention for measuring time. Unfortunately, this formulation of the law provides no independent test of the absence of force, as was pointed out by Bertrand Russell,[1] and therefore involves us in a circular definition.

Following the investigations of Hertz, the greatest contribution to the theory that the law of inertia is a convention was made by Poincaré,[2] who pointed out that: (*a*) there is no absolute space and we can conceive only of relative motions, but usually the mechanical facts are enunciated as if there were absolute space to which to refer them; (*b*) there is no absolute time, because the assertion that two durations are equal has no meaning except by convention; (*c*) we have no direct intuition of the simultaneity of two events occurring in different places; and (*d*) Euclidean geometry is itself only a sort of convention of language, and mechanical facts may therefore be enunciated with reference to a non-Euclidean space.

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[1] B. Russell, *The Principles of Mathematics*, London, 1937, p. 482

[2] H. Poincaré, *Science and Hypothesis*, translated by G. B. Halsted in *The Foundations of Science*, New York, 1929, pp. 92 *et seq.*

Poincaré not only denied that the law of inertia is an *a priori* truth, but he also disputed that it is an experimental fact, because no one has ever been able to experiment on bodies withdrawn from the action of every force, and indeed there could be no means of deciding when a body was subjected to no force. When teachers of mechanics state that the principle of inertia is verified indirectly by its consequences, Poincaré maintains that they mean that it is possible to verify the consequences of a more general principle, namely that the movements of all the material molecules of the universe depend on differential equations of the second order. In his view, the law of inertia is *not imposed* on us *a priori ;* other laws would be quite as compatible with the principle of sufficient reason, so that the differential equations of motion could be of the first or of any order. Equally, the law is *not imposed* on us *a posteriori*, because it cannot be subjected to a decisive test. The principles of dynamics may at first have appeared to us as experimental truths, but we ultimately find that we are obliged to use them as definitions.

VII *The Law of Inertia is* ‘ *Synthetic* a Priori ’ *in Function.* Assuming now that the law of inertia is, at least partly, a convention, it is natural to ask why we choose this particular convention rather than another. The very fact, however, that we ask this question implies that we have ceased to regard the law as a unique postulate about matter, but have come to regard it as a particular rule concerning scientific method. It has long been realised that the fundamental laws of physics do not all function in the same way. Newton [1] already emphasised the distinction between the laws of motion and the law of gravitation, as he conceived it : ‘ The *Vis inertiae* is a passive Principle by which Bodies persist in their Motion, or Rest, receive Motion in proportion to the Force impressing it and resist as much as they are resisted. By this Principle alone there never could have been any Motion in the World. Some other principle was necessary for putting the Bodies into Motion ; and now they are in Motion, some other Principle is necessary for conserving the Motion.’

Some modern natural philosophers, for example Pap,[2] believe that the distinction between the law of inertia and the law of gravitation lies in the fact that the former is a conditional law, and that scientific laws of conditional form ultimately tend to function as definitions. Illustrating this process, Pap quotes the case of Boyle’s law. Physicists found by experiment that some gases do not liquefy unless they are submitted to enormous pressures; they extrapolated and constructed the concept of an ideal gas which is in principle incapable of liquefaction. After the discovery of Boyle's law, an ideal gas came to be defined as any gas which satisfies this law. Often, as Pap remarks, when inductive generalisations become increasingly confirmed they tend to be used as principles by which phenomena are interpreted. Referring to the laws of classical dynamics, Pap reminds us that, in order to retain them in analysing the motion of the moon, the definition of equal times in terms of equal angular displacements of the earth was abandoned.

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[1] I. Newton, *Opticks,* 4th ed., London, 1730 ; reprinted, London, 1931, p. 397

[2] A. Pap, *The A Priori in Physical Theory,* New York, 1946, p. 25

I agree in principle with Pap's view that the law of inertia should be regarded as 'synthetic *a priori*' in the *functional* sense of defining, jointly with the other laws of motion, a method of analysing motions.[1] This view derives from Cassirer and from C. I. Lewis, who in *Mind and the World Order*,[2] wrote: 'The *a priori* element in natural sciences goes much deeper than might be supposed. All order of sufficient importance to be worthy of the name of law depends eventually upon some ordering of mind. Without initial principles by which we guide our attack upon the welter of experience, it would remain forever chaotic and refractory. In every science there are fundamental laws which are *a priori* because they formulate just such definite concepts of categorical tests by which alone investigation becomes possible.'

3 *The Class of Inertial Frames*

The difficulties which Hertz encountered in examining the traditional foundations of dynamics, or to use his own term, the traditional image, led him to consider possible alternative images, in particular the image associated with the doctrine of energy, which in his day had not yet been fully developed as a form of dynamics. His objection to the use of Hamilton's principle as a fundamental law, however, was its complexity: not only does this principle make the present motion depend on consequences which can only exhibit themselves in the future, thereby attributing intentions to inanimate nature, but these intentions are meaningless because the integral whose minimum is required by Hamilton's principle has no simple physical meaning. Although we cannot *a priori* assume that nature is ' simple ', we can insist that, if our images are well chosen, the actual relations between the objects must be represented by simple relations between the images. Hertz believed that, if the contents of the fundamental law are correct and comprehensive, it can be stated in a simple form by a suitable choice of the fundamental conceptions.

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[1] The problem of deciding *why* we proceed in this way is not fully investigated by Pap. See, however, for a detailed discussion on this question: G. J. Whitrow, *Philosophy*, 1946, **21**, 5

[2] C. I. Lewis, *Mind and the World Order*, New York, 1929, p. 254

Following this line of argument, we are led to ask whether we ought to regard the law of inertia as an axiom or a theorem. As I have pointed out elsewhere,[1] there is a significant analogy between the status of the axiom of parallels in Euclidean geometry and the law of inertia in dynamics. Euclidean geometry can be regarded as the particular geometry obeying the Helmholtz-Lie criteria of homogeneity, isotropy, etc., for which the curvature is zero. From this point of view the parallel ' postulate ' becomes a theorem. This reformulation of geometry as an ideal science of congruent measurement, rather than as a unique description of the spatial aspect of nature, suggests a corresponding reformulation of dynamics.

Such a reformulation has been attempted by Milne.[2] Following Mach,[3] he regards inertia not as an intrinsic property or attribute of each particle of matter, but rather as a relation between each particle and the whole system of masses of the universe. Two important consequences of this theory must be emphasised: (*a*) the aggregate of masses of the universe must be idealised in some way, for the fine-scale distribution is so complex that it is difficult to envisage how any uniform relationship of the kind contemplated in the law of inertia could be associated with it, and of course actual motions are, in general, non-uniform ; (*b*) given a suitable uniform material ' background ' we can derive a clear-cut definition of an inertial frame, namely one which is at rest relative to the local background. Milne has shown that with suitable conventions the motion of a ' free particle ' can be regarded as uniform relative to the background, and in this way has established a reformulated law of inertia as a *theorem* of dynamics. This analysis has led to new developments in the theory of time-scales, but in the present context the most important consequence is that it suggests a new distinction between different classes of inertial frame : those at rest relative to the local 'background', and those moving uniformly relative to it in terms of the appropriate scale of time.

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[1] G. J. Whitrow, *Philosophy*, 1946, **21**, 5.

[2] E. A. Milne, *Kinematic Relativity*, Oxford, 1948

[3] E. Mach, *The Science of Mechanics*, translated by T. J. McCormack, Chicago, 1893

The principle of relativity, both in its classical Newtonian form and in Einstein's *Special Theory*, presupposes that important classes of phenomena can be isolated and treated as if they were unaffected by the rest of the universe. It asserts that there are ∞^6 systems in which phenomena run in the same way irrespective of the relative motion of these systems. This principle is in accord with the negative results of experiments designed to detect 'absolute motion'. These experiments, of course, relate only to linear motion ; absolute rotational motion, however, can be detected. If we inhabited a planet, possibly like Venus, which was surrounded by impenetrable clouds, we believe that with the aid of a Foucault pendulum we could decide whether or not to regard our world as rotating, and that if the clouds were to evaporate we should find that the results of our experiments agreed with our observations of the fixed stars. As Bridgman [1] pointed out more than twenty years ago, this belief entails our rejection of the 'principle of isolation'. He maintained that the act of isolation could not be performed and that the rest of the universe could not be disregarded ; for in the problem of rotation there is no question of 'limiting behaviour', as arises in the traditional discussion of uniform motion.

The peculiar character of rotational motion has long been recognised, but the outstanding merit and originality of Bridgman's discussion was his realisation that a similar difficulty must apply in principle to translational motion, although in this case the corresponding effects may be too small to be detected, at least at present. He suggested that a physical basis for such a difference between rotation and translation might be found in the enormously different numerical values of translational and rotational 'velocities' attainable in practice. Whereas we may regard 2π as a natural unit of rotation, Bridgman proposed tentatively that the diameter of the galaxy might be the corresponding natural cosmic unit of translation. This particular length is perhaps less significant than he supposed, and probably a better choice would be either R or $2\pi R$, where R is of the order of a few times 10^9 light-years, the figure often suggested for the radius of the universe. Measured in such cosmic units the angular 'velocities' attainable in practice are far greater than the linear

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[1] P. W. Bridgman, *The Logic of Modern Physics*, New York, 1927, pp. 180 *et seq.*

‘ velocities ’. Bridgman suggests that the real state of affairs may be as follows : ‘ Phenomena in any system are affected by motion with respect to the entire universe, whether that motion is one of translation or rotation, and the magnitude of the effect is connected with the velocity of motion by a factor which is of the general order of unity when velocity is measured in cosmic units ’. The linear ‘ velocities ’ attainable in practice are so low that their effect has not yet been detected experimentally, but angular ‘ velocities ’ are high, and their effect is easily demonstrated. In Bridgman’s view ‘ the special theory of relativity is no different in character from any other physical law ; it is only approximate, and some day our measurements may become refined enough to detect its limitations ’.

Such an argument leads to a general endorsement of the view that in cosmical dynamics we must distinguish between two classes of inertial frames, primary and secondary, corresponding to the fundamental and the free particles of Kinematic Relativity.[1] So far, the consequences of this novel conception have only been partially explored, and its ultimate significance is still a matter for conjecture ; but, even if it proves to be far less revolutionary than was Galileo’s conception of dynamics, nevertheless we too may be witnessing the birth of ‘ a very new science dealing with a very ancient subject ’.

G. J. Whitrow

[1] E. A. Milne and G. J. Whitrow, *Phil. Mag.*, 1949, 40, 1244

THE 'DRIVE' ELEMENT IN LIFE

I

If we look at living things quite simply and objectively we cannot but be struck by one feature of their activities, which seems to mark them off from anything inorganic. This is the active, persistent and regulatory nature of these activities. In this short article I shall try to illustrate and defend the thesis that there is common to all living things this basic element of directive striving, usually unconscious and blind, only rarely emerging into consciousness to become intelligently purposive. The master-end towards which the directive and persistent activities of the individual life converge is the completion of the life-cycle, including as a rule reproductive preparations for the repetition of the life-cycle in the ensuing generation. This thesis I stated on a former occasion in the following terms. Emphasising the essentially *active* nature of life, I wrote: 'Structuro-functional wholeness or integrity, and specific structure, are actively built up and maintained in the course of development, chiefly by the morphogenetic and behavioural activity of cells or groups of cells. If this integrity is disturbed by injury or adverse environmental influences, it is, so far as possible, restored by appropriate physiological or morphogenetic activities on the part of the organism and its cells, so that the normal state is restored or a new adaptive norm of structure and function set up. The organism actively seeks out and selects the substances necessary for its metabolism, or draws them from its stores. It actively seeks in many cases its appropriate environment, and strives to maintain itself therein; it actively seeks in many cases a suitable ecological niche for its eggs and offspring. In all these ways, and in many others, the organism strives to persist in its own being, and to reach its normal completion or actualisation. This striving is not as a rule a conscious one, nor is there often any foresight of the end, but it exists all the same, as the very core of the organism's being.'[1]

An array of facts was set out in the book containing the passage in justification of this thesis, and I shall not attempt to repeat or summarise them. Instead, I shall adduce some further instances in support, and then consider briefly what may be the relation between our own experience of striving and the unconscious drive-like, ‘ triebartig ’, character of organic activities, both morphogenetic and behavioural.

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[1] E. S. Russell, *The Directiveness of Organic Activities*, Cambridge, 1945, p. 190

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One aspect of the active striving nature of life is familiar under the guise of the ‘ struggle for existence ’. All good observers of animals and plants in their natural surroundings, from Charles Darwin downwards, have emphasised the strenuous character of the struggle for life and the struggle for reproduction. A classical example is the persistent growth towards the light of the trees and creepers in a tropical forest, vividly described by Hingston in the following passage : ‘ Like the trees, the lianas are also struggling to get upward. Too weak of themselves, they need the shoulders of others. Ever on the look out for some object to grip, they curl or twine themselves around their victims, or shoot forth roots that cling into the bark, or hook themselves to objects by curved spines, or stretch out arm-like projections that grip the sides of the stems and trunks. In the end by some contrivance or another they get up and free themselves into the light. They are in a new world and a transformation follows. The dried-up twisted leafless cables shoot out branches in every direction, a tangled mat develops with profusion of foliage, smothering the tree up which it has scrambled and extending into the mats and branches of its neighbours, covering perhaps a dozen of them with its leafy pall, and at last bursting into flower, the final purpose of its long climb.’ [1] This example illustrates nicely the double purpose of the climber’s struggle towards the light—to satisfy its own needs for growth and self-maintenance and to produce its seeds in suitable surroundings, in a word to complete or fulfil its normal life-cycle. It must be borne in mind that the struggle for existence and persistence is not primarily a struggle against other organisms, an internecine strife with neighbours, but essentially an individual struggle to obtain the means of life and to reproduce the race. There is inevitably competition, and the weak or unlucky go to the wall, but this is a consequence of the individual *conatus* or striving towards life and reproduction.

The active persistent nature of growth and development is particularly well marked in plants, and there are many familiar examples—the persistent and oft-repeated growth of the dandelion from a stubbed root, the way in which it forces itself up to the light through an inch or so of tarred foot-path, the shooting up of new branches from a truncated oak, the extraordinary way in which the roots of a birch or a pine burrow into and grip the most unpromising rock surfaces and afford precarious foothold for the tree. In his description of the Canadian scene in *The Transplanted*, Frederic Niven well illustrates this, when he writes, ' Still among boulders, boulders as big as cottages, were a few trees here and there, trees that spread long roots out to great length, roots that gripped the rock claw-like, and took small hold on dust in their crevices '.

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[1] R. W. G. Hingston, *A Naturalist in the Guiana Forest*, London, 1932, pp. 34-35

In animals the persistent and striving character of vital activities is shown most clearly in their behaviour, as in the drive to satisfy hunger or to mate. The strength of the sex drive is well known to us from personal experience, and the most casual observation of our domestic animals reveals the active, strenuous and continuing nature of their sexual and reproductive behaviour. Think of a dog pursuing a bitch on heat, or a broody hen intent on satisfying her drive to incubate a clutch of eggs. All this is familiar and obvious—so obvious that we are apt to forget its significance.

But in animals directive effort is manifest not only in their behavioural actions but also in their morphogenetic and physiological activities, which at all stages of the life-cycle maintain and restore norms of function and structure, and replace what is missing, as in the healing of wounds and in regeneration—within the limits of the possible. Such activities do not proceed smoothly and automatically, or with a machine-like rigidity towards a fixed end. They are, especially in difficult conditions, flexible, persistent and variable. This characteristic of persistency with varied effort is most conspicuous in behavioural action, of which many examples could be adduced. I have tried to summarise these special characteristics of organic directive activity as follows [1]—When the goal is reached, action ceases, but if it is not reached, action usually persists and is often varied, alternative modes of action being employed if the normal one fails to attain the goal. Accordingly the goal may be reached in different ways, the end-state or terminus of action being more constant than the mode of reaching it.

As the goals to which organic activities are directive and persistent are normally related to the main biological ends of development, self-maintenance and reproduction, the direction of these activities is towards the completion or fulfilment of the life-cycle. Life-cycle completion is indeed the master law governing all the activities of the organism, to which other laws of smaller scope, such as the law of need-satisfaction, are subordinate.

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[1] *The Directiveness of Organic Activities*, p. 110

In normal conditions the activities of the life-cycle proceed in a stereotyped fashion, repeating in detail the life-cycle of previous generations, though not without active effort, as in an animal's struggle to obtain food or the growth-effort of a plant to reach the light. But when conditions are difficult, and especially when there is operational interference by man, the stereotyped course of life-cycle activities is upset, and adaptive responses become necessary if norms are to be restored and the life-cycle completed. In many organisms, if not in all, such adaptive or directive reactions to unusual or even unprecedented situations are forthcoming, though in different degree in different organisms.

Even in the earliest stages of embryogeny, if differentiation has not proceeded too far, regulatory activities will lead to the production of a normal embryo from a half or a quarter of the segmenting egg. This in itself is perhaps not very remarkable, and may even be susceptible of explanation in dynamic terms, as Bertalanffy[1] maintains. But there are other phenomena of embryonic regulation which seem to prove the existence of a drive to restore normality from abnormal beginnings, even if the drive be not fully successful. A good example is that described by Holtfreter, who writes: 'Perhaps one of the most impressive illustrations of the puzzling complexity and at the same time of the "sensefulness" of the individuation process is provided by the following experiment. Several blastoporal lips from early gastrulae of *Amblystoma punctatum* were cut out and exposed for about 10 minutes to alkali, which caused them to disintegrate into a heap of single, amoeboid cells. The disorganisation was carried still further by stirring and intermingling the free cells by means of a glass needle. When the suspension fluid was subsequently neutralised, the cells reaggregated into one or several spherical bodies, the cellular arrangement of which was of course quite different from the original one in the gastrula. During the following days there occurred a certain amount of reorganisation, for instead of retaining their random distribution, the cells performed directed movements which led to their sorting out and regrouping into two germ layers; all mesodermal cells tended to disappear from surface positions and to slip into the interior of the bodies, while the endodermal cells, in competition with ectodermal cells, went to establish continuous, well oriented surface epithelia. The internal mass of mesoderm became further segregated into the distinctly separated tissues of notochord, somites and kidney. The latter formed long coils of nephric tubules provided with nephrostomes and surrounded by blood capillaries. The notochord cells, though not united into a single straight cord, were, on the other hand, not freely dispersed but appeared in the form of a continuous, slightly dendritic organ. Finally, the skeletal muscle cells were grouped into somites, the arrangement of which was, however, irregular.'[1] In this remarkable case, through directive cell-migrations, selective cellular adhesions and mutual inductive action, there was formed from quite abnormal beginnings an approximation to a normal organisation. The complex activities concerned have all the appearance of co-operative striving to reach a normal end in conditions quite unprecedented in the history of the race. The experiment recalls the somewhat similar phenomenon of the reconstitution of sponges from dissociated cells.

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[1] L. von Bertalanffy, *Das biologische Weltbild*, Bern, 1949, I.

In many organisms the loss of a part of the body or of an organ leads to its replacement by regeneration, restoring thus the structuro-functional integrity which is necessary for continued life and the completion of the life-cycle. The activities concerned may be very complex, involving not a simple replacement but a complete remodelling of the remaining parts in such a way as to re-constitute a new whole, as in ' morphallaxis ' in planarians and polychaets.

It is noteworthy that organs may be regenerated which are not likely to be lost or destroyed in the ordinary course of events, but have been removed experimentally by surgical operation. There is the classical case of the regeneration (from the upper edge of the iris) of the lens extirpated from the eye of certain species of newt, a case which led G. Wolff, who discovered it, to postulate the existence in living things of a primary, not evolved, power of ' purposive ' or directive response. It has even been shown that in certain mammals, after the complete removal by surgical operation of the ovaries, new ovaries can be formed from non-ovarian tissues, and that this is followed in some cases by normal pregnancy.[2] Such a response to the loss of the ovaries could never have occurred in the whole history of the race.

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[1] J. Holtfreter, in *Growth in relation to Differentiation and Morphogenesis, Society for Experimental Biology, Symposia*, **2**, Cambridge, 1948, p. 21

[2] Evidence summarised by F. Wood Jones, *Habit and Heritage*, London, 1943, pp. 33-35

Adaptive response, as in regeneration, compensatory hypertrophy and functional adaptation generally, is a phenomenon widespread in the organic realm, coming into play when the normal course of life-cycle activities is hindered or thwarted by unfavourable circumstances or by the experimental interference of man. It is shown in very varied degree, but the power is never completely absent.

In the life-cycle of most organisms adaptive response is directive towards satisfying the needs and restoring the norms of the individual itself, including its needs for reproduction. In highly organised insect societies, however, adaptive response may serve the needs, not of the individual insect, but of the colony or ‘ social organism ’ as a whole. The late W. M. Wheeler, one of the best philosophical biologists, in the interesting parallel he drew between the organism and the insect society, well illustrated this remarkable fact. ‘ If the worker personnel be removed from a young ant colony ’, he wrote, ‘ leaving only the fertile queen, we find that this insect, if provided with a sufficiently voluminous fat-body, will set to work and rear another brood, or in other words, regenerate the missing soma (of the colony). . . . On the other hand, if the queen alone be removed, one of the workers will often develop its ovaries and take on the egg-laying function of the queen. In ants such substitution queens, or gynaecoid workers, are not fertilised and are therefore unable to assume their mother's worker and queen-producing functions. The termites, however, show a remarkable provision for restituting both of the fertile parents of the colony from the so-called complemental males and females ’.[1] These complemental males and females are less developed forms of the ‘ royal ’ caste, and take on the reproductive functions of the true kings and queens when these are removed from the colony. In this way the maintenance, development and reproduction of the termite state is ensured.

How the needs of the hive are supplied in normal conditions by the directive activities of the worker-bees has been fully elucidated by the researches of K. von Frisch and his collaborators, and they have shown also in what a remarkable manner the workers overcome the difficulties in providing for these needs which they encounter when their normal procedure is drastically upset by experimental interference. In the ordinary course of events the workers go through a regular cycle of activities, which is closely correlated with the state of development of their salivary and wax glands. In the first period of a worker-bee's active life, up to about the 10th day, its main occupation is the care of the larvae, which it nourishes with the secretion from its highly developed salivary glands, together with honey and pollen which it draws from the storage cells in the combs. During its second period, from the 10th to the 20th day, the salivary glands atrophy, and the wax glands on the underside of the abdomen develop greatly. At this stage in the cycle it produces wax and builds cells as required, also it cleans the hive, stores the food brought in by the foraging workers, makes its first orientation flights, and for the last two or three days acts as a watcher at the threshold of the hive. In the third and last period of its life it takes on the duty of foraging for honey and pollen, and during this stage its wax glands atrophy.

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[1] W. M. Wheeler, *Essays on Philosophical Biology*, Cambridge, Mass., 1939, p. 19

By an ingenious method Rösch contrived to separate the young workers from the old gang ; all the bees were first driven into one half of the hive (A) and shut off from the other half (B) ; then the hive was turned round 180° ; the younger bees remained in half A, intent on their domestic duties, while the older bees flew off on foraging expeditions ; when they returned they flew to their accustomed entrance which however led them into the B half of the hive only. Thus half A was inhabited by younger workers only, while half B was the home of the older bees. The half containing the young bees was accordingly without foragers, and food stores were soon used up. After a couple of days some of the bees lay starving on the bottom of the hive, and some of the larvae had been torn out of their cells and sucked to satisfy the food-need of the young bees. But on the third day a remarkable change in behaviour was observed. Bees of only one to two weeks in age went out foraging and came back laden with food, this in spite of the fact that their salivary glands were fully developed and their normal task was the feeding of the brood. The needs of the hive and not their stage of bodily organisation determined this adaptive modification of their normal behaviour ; their salivary glands rapidly atrophied.

The older bees based on half B had been deprived of the younger stock that normally act as nurses and feeders of the larvae, but some of them that still had salivary glands in a functional state stepped into the breach and fed the young, retaining their glands in action long beyond the normal time. Here again the needs of the hive were attended to, rather than individual needs.

In another experiment by Rösch a hive was robbed of its building workers, and put in a situation where the construction of new comb became urgently necessary. The building of new cells was undertaken by bees that had passed the normal age for this task and whose wax glands were in course of disappearing. The remarkable fact came to light, through microscopical examination, that in these bees the fat-body had pressed itself up against the reduced wax glands and caused them to re-develop. K. von Frisch, from whose summary I take this account of Rösch's experiments, makes an interesting remark apropos of this last finding, to the effect that 'Man ist versucht, zu sagen: Der Wille regiert den Körper'. But he cautiously adds, 'Doch wir wissen nichts vom Willen der Biene und lassen das Rätsel ungelöst'.[1]

3

We have now surveyed a number of examples, drawn from quite different fields, which illustrate what I have called the active, persistent and regulatory character of organic activities, as shown particularly in adaptive response, whether behavioural or morphogenetic. Now we as conscious subjects have direct and immediate knowledge or experience of this element of drive or striving towards biological ends which appears from objective evidence to be a characteristic common to all or many organic activities. We are part of Nature, and there must therefore be some relation between our experienced striving and that observed in other organisms. What then can be the relation between this experienced drive and the objective 'drive-character' of organic activities in general? Must we not think of them as two aspects—internal and external—of the same basic reality? Must we not postulate a general, supra-individual hormé or drive in life, after the fashion of Schopenhauer's 'Wille'? To do so would be to go beyond the bounds of science into the realm of metaphysical speculation. It is obviously impossible in a short essay to follow up and discuss this hormic theory of life, and I shall content myself with a few observations.

It is clear that there is nothing to be gained by trying to interpret morphogenetic and physiological activities in psychological terms such as are applicable to our own activity as conscious subjects. It is possible, within limits, to interpret the behaviour of animals in such terms, as Bierens de Haan[2] in particular has shown, but this mode of interpretation becomes increasingly doubtful and uncertain when we try to apply it to animals very different from ourselves, and appears highly hypothetical if applied to morphogenetic activities, though this attempt has been made by Agar.[1] Yet it seems certain that there is some element or factor in organic activities generally which cannot be a property of a purely physical system, and our only clue to its nature is that it must in some sense be psychological.[2] We know that we ourselves are psycho-physical unities, and certainly not purely physical systems, and we have every right to extend this conception to organisms in general.

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[1] K. von Frisch, *Zts. f. Tierpsychol.*, 1937, 1, 12

[2] J. A. Bierens de Haan, *Die tierischen Instinkte*, Leiden, 1940

We have a clue to the nature of directive activity in our own experience of striving. This is only a partial clue, and it is liable to be a misleading one, for our behavioural activity, which alone is directly known to us, is for the most part of a unique kind, not found elsewhere in the organic realm. It is often guided by conceptual thought; we pursue ends already explicitly present in consciousness, and our behaviour is to that extent intelligently purposive. But by no means all our behaviour-life reaches this purposive level; much of it is rooted in unconscious desires and impulses, closely related with our bodily and physiological state, as for instance in the hunger or the sex drive. Such instinctive and often unconscious drives we share with other animals, and these drives, we know, are not purely psychical but psycho-physical. We have therefore *some* direct insight into the nature of one form of directive activity, namely instinctive behaviour. Now there is, as I have pointed out in my *Directiveness of Organic Activities*, a close analogy between instinctive behaviour and directive activity in general. They have the same characteristics of persistency with varied effort towards reaching goals that are normally related to biological ends, without foreknowledge of those ends. The conclusion therefore seems reasonable that in directive activity generally, and not only in instinctive behaviour, there must be an element which we can conceive only as psychological, as a function of a psycho-physical organism.

As I have already said, this is not the occasion to discuss the very difficult question of the nature of organism and of directive activity. I have been concerned only to indicate a possible line of approach.

E. S. Russell

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[1] W. E. Agar, *A Contribution to the Theory of the Living Organism*, Melbourne, 1943

[2] See R. S. Lillie, *General Biology and Philosophy of Organism*, Chicago, 1945

INDETERMINISM IN QUANTUM PHYSICS AND IN CLASSICAL PHYSICS[1]

PART I

I

Before entering into a more detailed discussion, I shall attempt, in this introductory section, to state my main points in outline.

Quantum physics is now generally admitted to be indeterministic in the sense that it implies the impossibility of predicting certain kinds of physical events, however complete our initial information may be concerning the physical system in question ; given sufficiently precise initial information we may, however, predict the probability of these events, i.e. the frequency of their occurrence under sufficiently similar conditions. Classical physics, on the other hand, is usually taken to be deterministic in the sense that it implies the predictability, with any desired degree of precision, of every single physical event, on the basis of sufficiently precise initial information. In the present paper I propose to show that the opposition indicated here is misleading even although the *prima facie* deterministic character of classical physics must be admitted. In spite of important differences, the situation in classical physics shows greater similarities to that in quantum physics than is usually believed. My thesis is that most systems of physics, including classical physics and quantum physics, are indeterministic in perhaps an even more fundamental sense than the one usually ascribed to the indeterminism of quantum physics (in so far as the unpredictability of the events which we shall consider is not mitigated by the predictability of their frequencies).

The impossibility, implied by quantum physics, of predicting events of a certain kind is an impossibility of a peculiar character. If we assert of an observable event that it is unpredictable we do not mean, of course, that it is logically or physically impossible for anybody to give a correct description of the event in question before it has occurred; for it is clearly not impossible that somebody may hit upon such a description accidentally. What is asserted is that certain rational methods of prediction break down in certain cases—the methods of prediction which are practised in physical science. These methods involve various physical processes, among them those of obtaining initial information by way of observation. It is the analysis of these latter processes in the light of quantum physics which establishes the impossibility of obtaining predictions in the cases in question. The impossibility asserted is therefore a physical impossibility—that of successfully carrying out certain physical operations involved in obtaining predictions in accordance with the methods of science.

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[1] Expanded version of a paper read before the Philosophy of Science Group of the British Society for the History of Science, at their first Ordinary Meeting on November 15th, 1948.

It is usually asserted (I think correctly) that the physical processes involved in observation lead to difficulties only in quantum physics, and that they disappear if it is assumed that Planck's quantum of action equals zero—in other words, in classical physics. Nevertheless I contend that if, in a similar way, we analyse besides the physical processes involved in observation certain other physical processes which are also involved in every prediction, then we find a somewhat similar result which, however, is valid for classical physics also. The processes which our analysis must take into account besides observation are the physical processes involved in the calculation and formulation of the predictions. If this is done then we find that all scientific predictions are in an important sense deficient, including the most complete set of predictions that is physically possible to formulate, even from the point of view of classical physics ; and we find that this deficiency becomes significant whenever we wish to predict the behaviour of physical systems (classical or otherwise) of a certain kind, viz, of predicting machines.

Our procedure will be to consider some of the properties, and especially the limitations, of what we shall call a ' predictor ', i.e. a classical mechanical calculating and predicting machine which is so constructed as to produce permanent records of some kind (such as a tape with holes punched into it) capable of being interpreted as predictions of the positions, velocities, and masses of physical particles. It will be shown that such a machine can never fully predict every one of its own future states, and consequently not those of what we shall call its (closer) ' environment ', i.e. the part of the world with which it (strongly) interacts. Such a machine, moreover, can never fully predict, or be predicted by, any sufficiently similar machine with which it interacts. Thus if we call a set of similar and interacting predictors a ' society ', then we can say that no member of such a society can fully predict the future states of that society, or those of any of its members.

All this follows very simply from the general idea of a physical predictor. But if we further take into account the implications of either classical particle mechanics or of certain other classical systems of physics then we even find that the following alternative holds for every classical physical world. Either no predictor exists in the world in question, and consequently no predictions in a physical sense, or the future states of one at least of the existing predictors cannot be predicted by any of the existing predictors.

But this implies the physical impossibility of predicting, with the help of the methods of science, certain physical events ; or, in other words, it implies an indeterminism of a kind somewhat similar to the one implied by quantum physics although, it seems, unmitigated by the predictability of probabilities or frequencies.

Our argument is somewhat similar to certain ideas of Niels Bohr's who suggests repeatedly[1] that there is an ' impossibility of distinguishing, in introspection, sharply between subject and object ', and who connects this idea with indeterminism ; ' it must never be forgotten ', he writes,[2] ' that we ourselves are both actors and spectators in the drama of existence.' Bohr suggests that these considerations may be applicable to psychology, and that a kind of ' complementarity ' may exist between explanation and volition. Now our physical predictors, too, are actors and spectators, as it were. They compute ; and what they compute are descriptions. Nevertheless, our argument does not appear to be one of involving what Bohr has called ' complementarity '.[3] For complementarity is a symmetrical relation (if *a* is complementary to *b*, then *b* is complementary to *a*). Admittedly, the fact that a predictor can predict a ' society ' only if it does not become a member of it, has some resemblance to complementarity. But the more fundamental fact that a predictor cannot *predict* itself (although it may *explain*, as we shall see, its own actions after the event) does not appear to point to any other limitation which may be said to be complementary to it.

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[1] See for example, Niels Bohr, ' Biology and Atomic Physics ', *Celebrazione del Secondo Centenario della Nascita di Luigi Galvani, Congressi Scientifici*, Bologna, 1938, p. 14 ; and ' On the Notions of Causality and Complementarity ', *Dialectica*, 1948, **2**, 318. See also Gilbert Ryle, *The Concept of Mind*, London, 1949, pp. 195 *et seq.*

[2] Bohr, *loc. cit.* (*Dialectica*).

[3] To Bohr's distinction between ourselves as actors and as spectators (of ourselves) corresponds Ryle's distinction between ourselves as ' performing ' and as ' commenting ' (upon our performance). But this relation is not a complementary one since it is not symmetrical but hierarchical in character ; to comment upon a performance ' is to perform a higher order act '. (Ryle, *loc. cit.*, p. 195.)

2

We shall take indeterminism to be a doctrine asserting that *not all* events are 'determined in every detail' (whatever this may mean), and determinism as asserting that they *all* are, without exception, whether future, present, or past. It is necessary to be clear that the problem contested by the two doctrines is only [1] this, and that indeterminism does not, perhaps, assert that all or most or many events are not determined, but only that some events which are not completely determined exist—however rare they may be.

How the word 'determined' should be interpreted in this context is not quite easy to say. In fact, there exist a good number of possible interpretations and, accordingly, a number of different doctrines of determinism (and, of course, of indeterminism). Although we shall discuss in detail only one of these—the one which interprets the word 'determined' as 'predictable in accordance with the methods of science'—it may be of interest first to refer briefly to some of the other doctrines.

One can present the general idea of determinism as arising out of a critical revision of a commonsense view of the world—the view that some future events or occurrences (such as the apparent daily or yearly movements of the sun) are 'necessary' or 'predetermined' and therefore predictable, while others (such as the vagaries of the weather) are 'due to accident' and therefore unpredictable. A slightly more sophisticated view arises out of the doubt whether this classification is a valid one; whether the apparently 'accidental' events do not appear to us in this way merely because of our lack of knowledge; and whether we should not be able to predict them like the others if only we knew more about the conditions of their occurrence.

When this more sophisticated view develops into a doctrine, it may be called the 'metaphysical doctrine of determinism'. According to this doctrine, all events of this world are completely predetermined. While, to the commonsense view, there is a difference in the degree of 'fixedness', or alterability, between future and past events—the future is open, as it were, and future events are still alterable, while the past is unalterably fixed—metaphysical determinism believes that future events are, even before they happen, unalterably fixed, in exactly the same sense as past events are fixed. Accordingly, they might be foreknown—in exactly the same sense as they might be known after they have happened. Whether or not anybody has any such foreknowledge of them or any historical knowledge after the event, is, of course, a different question.

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[1] This is, for example, clearly expressed in P. W. Bridgman, *The Logic of Modern Physics*, New York, 1927, p. 210; while any such reference to *all* events is missing in the description of determinism in Max Born, *Natural Philosophy of Cause and Chance*, Cambridge, 1949, pp. 8 and 9. (See also the next note.)

But it ought to be realised that, should anybody possess any real foreknowledge of an event—not a mere guess—then this would imply that the event was predetermined. Accordingly, any doctrine asserting the existence or possibility of complete[1] foreknowledge implies the metaphysical doctrine of determinism. As J. S. Mill correctly observed, the doctrine of an omniscient being implies, for this reason, that of determinism (if by ' omniscience ' we mean the ability to answer with certainty every conceivable question, including questions concerning the future). For the question whether an event of a certain description will happen at a future date can be answered now with certainty only if it is now certain whether this event will or will not occur. It may be remarked that for this reason, the doctrine of omniscience contradicts that of omnipotence ; even if we take this latter doctrine in the very weak sense as merely implying that if an omnipotent being exists now, it has now some power, i.e. it can now alter the future course of events. For if we assume that the future course of events is now predetermined, then we also must assume that it is strictly impossible now to alter the course of events, and that every being is now completely powerless to achieve such a feat.

The metaphysical doctrine of indeterminism merely asserts that there exists at least one event (or perhaps, one kind of events, such as certain human activities, and events depending on them) which is not predetermined ; or that there is at least one question about the future which cannot be answered with certainty but must be ' left open '.

Both the metaphysical doctrines of determinism and indeterminism are, clearly, not testable. For even if the future would constantly surprise us, and show no sign of any predetermination, it might still be predetermined, and even foreknown—written in the book of destiny. On the other hand, even if everything had a completely regular and deterministic appearance, this would not establish that there is no single undetermined event. But lack of testability (in my opinion [1]) does neither here nor elsewhere establish lack of meaning, although it may be taken to establish lack of what might be called 'empirical content'. A doctrine may be logically too weak to be tested, and may yet be implied, just because of its weakness, by a testable doctrine. This contingency appears to be realised, more or less, in our case ; for the doctrine of determinism may be given a testable form, if we interpret determinism by scientific predictability. And it may be claimed that, in this form, it implies the metaphysical doctrine of determinism in the same way in which any doctrine of foreknowledge implies this metaphysical doctrine.

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[1] A doctrine (such as Born's) which merely asserts that ' events at different times are connected by laws in such a way that predictions of unknown situations . . . can be made ', without asserting that this holds, in principle, for *all* events, or all unknown situations, does not, of course, assert the possibility of *complete* foreknowledge.

3

The *prima facie* deterministic character of classical Newtonian mechanics is perhaps most strikingly illustrated by the story of the so-called '*Laplacean demon*'.

The 'Laplacean demon' is a superhuman intelligence capable of knowing the positions, masses, and velocities of all elementary particles (point-masses) for a certain moment of time t_0. Laplace pointed out that this knowledge (we shall call it 'initial information') of the t_0 state of the world together with a knowledge of Newtonian theory of mechanics would enable the demon to predict, by way of logical or mathematical deduction, every future state of the world. (It is thereby assumed that the problem of more than two gravitating bodies is soluble.)

One can argue that this kind of predictability is deterministic—even in a stronger sense than is the doctrine of metaphysical determinism. For since the Laplacean doctrine implies that a knowledge of present or past states involves the foreknowledge of any future state, all future states must be determined now. (Or at least, this appears to be implied by the laws of classical mechanics.)

The Laplacean doctrine of determinism replaces the vague idea of unalterability of fixedness by the much more precise idea of *predictability in accordance with a definite rational method*—the method of science which deduces predictions from initial information in connection with theories. This, no doubt, is an important improvement on the metaphysical doctrine. But the Laplacean doctrine is still too abstract to be testable. It demands an amount of initial knowledge which may turn out to be unobtainable since it may be infinite in several respects. For the doctrine speaks of the whole world which may contain an infinite number of particles ; and it assumes infinitely precise measurements.

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[1] Cf. my *Logik der Forschung*, Vienna, 1935, pp. 8 *et seq.*, 33, 74

One might say that all these difficulties arise from the fact that the story of the Laplacean demon is an attempt to eliminate the vague and dangerous phrase ' in principle '. For what it tries to explain is what we mean when we say that the future states of a system can be ' in principle ' predicted on the basis of a knowledge of past or present states. ' In principle ' means here something like ' not in practice, because human knowledge is never sufficiently precise and complete '. No wonder that, in attempting to explain what we mean by ' in principle ', Laplace was driven to introduce a superhuman intelligence. But the Laplacean demon is unsatisfactory, we may say, just because infinitely precise and complete knowledge is also ' in principle ' unattainable.

However, it appears that we may avoid the infinities and impossibilities involved in the Laplacean doctrine, and formulate a *finite version* of the doctrine of determinism.

Several steps must be taken in order to achieve this. The most important of them is to *embody* Laplace's disembodied spirit, that is to say, to make this spirit *a member of the physical world which it tries to predict.* We shall consider, accordingly, a ' predictor ' instead of a demon, i.e. a physical predicting machine. And we shall assume, in discussing classical mechanics, that this machine is a classical mechanism, subject to the laws of mechanics.

Such a machine will be able to obtain measurements of the masses, positions, and velocities of the physical particles within a certain finite spatial region—its ' environment '—for example, by using sensitive tentacles, and measuring rods, etc. It will be able to carry out a number of physical operations which we may interpret as calculations proceeding on this initial information in conjunction with physical theories and mathematical methods which, we may assume, are part of the total information with which the calculator is supplied. (This part of the information, we may assume, was built into the calculator by its makers.) And it will be able, ultimately, to produce predictions in the form of some permanent record such as a punched tape (or a typewritten report).

The part of the world which such a predictor can explore—its environment—must be finite. Accordingly, the predictor will be unable correctly to predict events within its environment which it might have predicted on the basis of information about conditions beyond its environment (we can call such events ' due to interference from outside '). Similarly, the measurements it can take, and the predictions it can produce, will be at best of a certain finite degree of precision. As a consequence, we can expect that predictions about the near future will be more successful than predictions about the more distant future.

We can now introduce the auxiliary idea of a ' *specified finite prediction task* '. By this we mean *the task of predicting, with some chosen and specified degree of precision, some event—i.e. the position and/or velocity of its particles—occurring in a finite mechanical system, sufficiently isolated from outside interference, at a certain chosen future instant of time.*

We can make this idea more definite still, for example, by specifying limits to the period of time within which the instant may be chosen for the event to be predicted. We may also, perhaps, specify an upper limit for the size of the physical system (especially if we are interested in such special problems as that of human determinism). And we may, moreover, specify more precisely that, and under which circumstances, an unsuccessful prediction is not to be counted as a negative instance if we find evidence, after the event, that it may be accounted for (on the basis of the theories used) as due to interference from outside, or in other words, to insufficient isolation. However, the problem of interference from outside shall not be considered here in more detail.

With the help of this, we may now formulate our finite version of the deterministic doctrine.

For any specified finite prediction task, it is physically possible to construct a predictor capable of carrying out this task. (' Physically possible ' means ' possible from the point of view of the system of physics under consideration '—for example, classical mechanics.)

Since, for every specified degree of precision of the prediction, there will be a corresponding (often higher) degree of precision of the necessary initial measurements, this formulation implies that it is physically possible to construct a predictor capable of obtaining initial information with any specified finite degree of precision (i.e. short of absolute precision).

The finite version here given is an attempt to formulate that doctrine of determinism which is rejected by quantum physics, and which, as I shall try to show, is also incompatible with most classical systems, for different although perhaps somewhat similar reasons.

4

Our formulation of the finite doctrine refers essentially to some system of physical theory. Such a system is assumed to be incorporated in the predictor ; besides, it must be used when we construct a predictor capable of carrying out the specified task. (And ultimately, it must be used when determining whether the degree of isolation of the system was sufficient.)

It may be thought that this reference to a certain theory is unnecessary, and that an alternative finite formulation may be given which is free from it, as follows : The states of two sufficiently isolated physical systems whose states at the instant of time t_0 agree with a sufficient degree of precision will, at any later instant t_1, agree with any specified degree of precision.

A very similar further alternative would be the formulation: every physical event in a system can be reproduced, by way of reproducing in another system one of the states which preceded the event in question.

Both these formulations appear to be independent of any reference to a physical theory ; but this appearance is misleading. For the ideas of a physical event, and of a physical state, etc., are dependent upon the theory chosen. (For example, whether an event or state may be characterised by stating, apart from positions, the velocities, or whether time derivatives of a higher order will have to be obtained, depends upon the theory—whether its differential equations are of the second or a higher order.) To put it in another way : without theory we can have no idea whether two systems which ‘ look ’ very similar are physically similar or not, and therefore, what the degree of their similarity is.

We could of course always say, if the two systems turned out to become obviously dissimilar in the course of time, that they were not in the same state to start with. But this would mean *assuming* a deterministic principle ; and in such a way that it could never be tested.

An example may be helpful. We may build two clocks so similar that we cannot detect a difference. We shall expect, nevertheless, that they will show a deviation after the lapse of some time. If they can be ' regulated ' by some mechanism, we can reduce this deviation—but as a rule at the cost of introducing a clearly visible difference into the position of the two regulator mechanisms. Thus, in practice, we do not proceed in the way described by our alternative formulations ; but we proceed in full accordance with our theories which make us expect that the degree of similarity of the two clocks will be insufficient for certain purposes.

If, however, we keep in mind that the two alternative formulations given in this section are to be taken as relative to a theory which determines what is, and what is not, a similar state, then we can accept them as equivalent to our previous finite version, provided we remember that this similarity is to be determined by a physical predictor. This would lead to a reformulation of our latest version along lines such as these :

For every physical event, there exists a predictor (*i.e. it is physically possible to construct a predictor*) *capable of reproducing the event in another system by way of reproducing one of the states which preceded the event in question.*

This formulation extends the idea of a ' predictor ' to that of a ' reproducer ', but I do not think that this extension means an important alteration in our approach.[1]

5

Our finite version, by postulating the existence of a predictor (i.e. the physical possibility of its construction) capable of carrying out prediction tasks with any specified precision, implies that every predictor can be improved, in the sense that it is always physically possible to construct one whose measurements are more precise than any given one. Or, in other words, predictors may be constructed which approximate more and more closely to a perfect predictor capable of absolutely precise measurements.

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[1] I do not discuss a very different doctrine of determinism which may be called ' programmatic determinism ' and which may or may not be connected with a belief in the metaphysical doctrine. I have in mind the doctrine (which, it appears, is held for example by Einstein) that, even if our present physical theories do not support determinism, we shall advance, in time, to the discovery of a completely deterministic system of physical theories. Yet it is not, I believe, this programme, but rather the alleged deterministic character of definite theories (such as classical particle mechanics) which creates a serious problem of the philosophy of science ; only the belief that such physical theories actually exist, and that they have been successful, gives plausibility to the hopes of the programmatic determinist. (That this fact is neglected, and that his criticism confines itself to what I call ' programmatic determinism ', is the reason why I do not think that Professor Ryle's otherwise brilliant comments on the problem are completely satisfactory ; see Gilbert Ryle, *The Concept of Mind*, pp. 76–81.)

In connection with later discussions, it is important to realise two points, viz, (1) that this series of predictors is in a definite sense convergent, in so far as their achievements—the degree of precision—converges ; (2) that our postulate does not imply the actual co-existence of this infinite series of predictors, but only the possibility of constructing one for every given prediction task.

6

So much about the finite version of the deterministic doctrine which we shall attempt to refute. We now turn to its refutation.

Quantum physics implies the denial of what I have called the finite version. It implies that, however full and precise the initial information obtained, and however well isolated the system in question, there are certain physical events which cannot be predicted, although it is possible to predict the frequency of their occurrence under like conditions. Thus certain finite specified prediction tasks cannot be carried out ; and the most perfect predictor which can be constructed cannot reproduce every event, even although it can reproduce every system in such a way that the two systems will coincide in respect of the frequencies of the occurrence of the event in question.

Quantum physics tries to elucidate this result by the assertion that the system, although isolated from outside interference, cannot be completely isolated from what I shall call ‘ interference from within ’, viz, from the very centre of the predictor's environment—from the predictor itself. In obtaining initial information, the predictor must interact with the system in question, and this interaction introduces into the system a disturbance whose magnitude is unpredictable within a certain halo of uncertainty related to Planck's quantum of action h. The uncertainty relation can be written

$$\Delta q \Delta p \geq h/4\pi$$

where Δq and Δp are uncertainties of the position and momentum. From this we can see that the uncertainty becomes insignificant for sufficiently great masses. We see also that, if we assume that no quantum of action exists—or in other words, if we assume that $h = 0$—then the uncertainty disappears. Now this last assumption—the absence of a quantum of action—is the distinguishing characteristic of classical physics from the point of view of quantum physics. This is why quantum physicists usually assert that classical physics is deterministic. And indeed, it does not know this kind of indeterminacy.

But I shall try to show that classical physics (and indeed, most if not all systems of physics) knows a similar kind of indeterminacy, also due to 'interference from within.' It may be formulated in this way.

Although, in the absence of Planck's quantum of action, the interference between a predictor P^+ and most systems may be made as small as we like, this is not true for all systems, and especially not if the system under consideration is, in its turn, a predictor P, attempting to predict P^+. Nor is there any reason to believe that it is physically possible, from the point of view of classical mechanics, to construct for every predictor P a predictor P^+ sufficiently superior to be capable of predicting P—not even if we assume (as we did in section 5) that for every predictor, however good, it is possible to construct a better one.

The system which P^+ cannot predict need not be a predictor P. In general, that part of the environment of P^+ with which P^+ may, by its construction, strongly interfere, will be unpredictable for P^+ because, as we shall see, P^+ cannot predict every one of its own future states. Nevertheless P^+ may be constructed, *in the cases ordinarily considered*, in such a way that it cannot interfere strongly with the system under consideration ; or more precisely, in the absence of h, P may be so constructed as to interfere as little as we like, with an 'ordinary' system. Our thesis is, simply, that even classical mechanics implies that there are limits to this procedure, and that there exist finite specified classical mechanical prediction tasks which no classical mechanical predictor can perform.

This is our thesis. We proceed to establish it.

7

We shall first discuss the situation in a general way.

Consider a mechanical system A, not containing a predictor, and a predictor B attempting to predict A. This it can do only if (1) B can calculate the results of its interference with A or if (2) B interferes sufficiently weakly with A.

As to (1), B can assess the way in which it interferes with A either by studying its interfering parts B' and their interaction with A—which means that it has to study the system $A + B'$ instead of A—or on the basis of predictions about itself (without splitting off some of its parts). The first of these two alternatives does not help, for in order to obtain the necessary information about $A + B'$, the whole problem arises again. The second alternative also does not help ; for it will be shown in the next section (8) that a predictor cannot have such knowledge about itself.

Thus we must consider the alternative (2). This means, in effect, that B is so constructed as to interfere only very weakly with systems such as A ; and it can ' know ' this fact, as it were, by implication ; for example, by being so constructed that its predictions of A neglect its interference with A.

Thus B interferes only weakly with A. On the other hand, A must, under certain circumstances, interfere strongly with B. It must do so because some minute differences at the instant of time t_0 may, in a mechanical system such as A, give rise to considerable differences at the instant of time t_1 ; but since B is a predictor trying to predict the t_1 state of A, it must be able to react in very different ways to the two minutely different states of A.

We can express this situation metaphorically by saying that we can consider A to be separated from B by something like a ' one-way membrane ' which allows only weak influences to pass from A to B, but strong ones in the other direction. We can also say that, if B is a predictor, it must be capable of amplifying certain weak effects reaching it from A. Or in other words, a predictor B must be an amplifier simply because some small differences in the system A may become amplified in A itself in the course of time ; and the predictor B must be capable of producing a corresponding amplification.[1]

This description of ours, of the way in which A and B interact, may be said to be undertaken from the point of view of a second predictor C which studies this interaction, i.e. the system $A + B$. C can predict, by measuring the state of A and of B, not only future states of A but also the predictions of that state (the tape) which B will produce ; and if C is a better predictor than B, it may even be able to predict how much B interferes with A, and perhaps the extent to which this gives rise to errors in the predictions produced by B.

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[1] The argument is intended to show that predictors must be amplifiers *qua* predictors. But it might also be shown, I believe, that they must be amplifiers merely as recorders of measurements ; for they must be capable of recording minute differences by the same method (such as the punching of holes) as large ones.

All this will be possible only if C interferes weakly with the system $A + B$, or in other words, if there is again something like a 'one-way membrane' between $A + B$ and C, which allows strong influences to pass in only one direction—from $A + B$ to C.

Now we assume that B is similar to C in a degree sufficient to reciprocate C's interest in B, i.e. that B begins to study the state of C in a way similar to that in which C is studying the state of B. Then B will amplify certain influences coming from C, just as C amplifies certain influences coming from B. Accordingly, the 'one-way membrane' between B and C breaks down, and with it the conditions for successful prediction. Neither can B predict C, nor can C predict B.

This simple situation is of decisive significance for our problem. It shows that if the system to be predicted has certain characteristics—in the main, that it amplifies the otherwise weak disturbances which it suffers from the predictor—then the predicting task cannot be carried out.

If we analyse this situation more clearly, then we find that it depends on the fact (to be discussed in the next section) that neither of the machines can have knowledge of its own state before that state has passed; in fact, it can obtain information about its own state only by way of studying the results obtained by another machine, or by being given these results (say by way of a tape which may pass from one machine to another). In our case, this means that B, for example, can know about its own state only by studying the state of C, or by being interfered with by C's tape. B, therefore, cannot allow for the influence of its own state upon C for the purpose of predicting C; and *vice versa*.

All this holds, of course, not only for B and C but for any set of sufficiently similar and interacting predictors—for a 'society' of predictors, as we called such a set in section 1. None of the members of such a society can predict the future state of its members in every detail; and accordingly, none can predict the future state of the physical system of which it is a part—of the 'society.'

Our analysis of the system $B + C$ can be said to proceed from the point of view of an outsider, the predictor D. As long as D is outside the system—that is to say, as long as D is in the rôle of a demon—it may predict precisely the way in which B and C disturb each other, and in which they fail accordingly to produce successful predictions. But once *B* or *C* or both ' discover ' *D* (or the instruments used by *D* for measuring the system $B + C$), and try to predict it, *D* will be unable to predict the system $B + C$, since the future behaviour of the system $B + C$ will become dependent upon the behaviour of *D* which *D* itself cannot predict ; thus *D* will become a member of the ' society ' to which *A* and *B* belong.

Now it is, of course, of the greatest importance to realise under which circumstances *D* can remain ' outside ' of the society of *A* and *B*, and under which circumstances it becomes a member of this society.

If, as we assumed, *B* and *C* are mechanical systems, then *D* may, for example, remain a ' demon ' relative to *B* and *C* if it is an electro-magnetic predictor, observing $B + C$ by, say, optical means, or by radar (whose disturbing mechanical effects may be made as small as one likes, from the classical point of view). It then may possibly possess no moving parts, so that *B* and *C*, when studying D, will both simply predict that *D* does not move. These predictions can be easily anticipated by *D* when it has found out the mechanism of *B* and *C*. (D operates electro-magnetically, but is in possession of the theory of mechanics.) *D*, as we have described it here, is a kind of realisation of the Laplacean demon, in so far as it really does not belong to the ' world ' which it predicts ; it transcends the mechanical world. Such a realisation can, however, exist in a physical sense, only because the system of classical mechanics which it transcends is physically incomplete. In other words, were classical mechanics capable of explaining electro-magnetism as due to the movements of mechanical particles, then an electro-mechanical *D* would lose the character of being a demon.

Thus *D*'s capacity of predicting $B + C$ is due to the fact that classical mechanics is insufficient as a theory of the physical world, and that a purely mechanical determinism, accordingly, is refutable. (There are miracles in a purely mechanical world, as it were,—electro-magnetic miracles.)

We may ask whether *D* might not be a purely mechanical predictor and still remain ' outside ' the society of any *B* and *C*—especially in view of section 5 where we have postulated that there exists, to every predictor, a superior one. The answer to this question turns out to be negative, even though it must be admitted that there are degrees in the membership of a ' society ' and that *D* might be a kind of semi-member of *C* and *B*—sufficiently superior in its structure to be a little more successful in predicting *C* and *B* than these are in predicting *D*. (That such ' degrees ' of membership are possible follows from the fact that *D* may be a mixed electrical and mechanical system.)

Upon the assumption of determinism (section 3) that to every prediction task there exists an appropriate predictor, there must exist mechanical predictors *B* and *C* capable of measuring, with an appreciable degree of precision, all the individual particles of a mechanical system *A* (a gas, for example). But if this is so, then it is clear that a *mechanical* predictor *D* could escape ' detection ' by *B* and *C* only if *D* is outside the reach of the instruments (tentacles) of *B* and *C*. But this is not possible ; at least some mechanical parts of *D* must operate within the reach of *B* and *C*, and in attempting to predict the movements of these parts, *B* and *C* will strongly react to these movements. They may be completely unsuccessful, and this fact, for instance, may be ' known ' to a sufficiently superior *D*, while only little about *D* may be ' known ' to *B* or *C*. But since *D* cannot predict in detail the movements of its own parts, it cannot predict in detail the movements by which *B* and *C* react to the movements of its parts.

Of course, if *B*'s and *C*'s ' attention ' are otherwise engaged than in attempting to predict *D*, or parts of *D*, then *D* may be more successful in predicting *B* and *C*.

These considerations assume classical particle mechanics—some kind of atomism without quanta. But even if we assume a continuity system of mechanics—with infinitely divisible material substances—even then would we obtain similar results. For in such a system we would still have to assume, for the parts (say, the measuring rods or tentacles) of a mechanical *D*, that there must be a lower limit to their thickness or mass, etc., since otherwise they would become too easily distorted. Or to put it in another way : in a mechanical world, no *D* can be at the same time effective and vanishingly small.

But even if one mechanical system *D* did exist which could escape detection ' by *B* and *C*, this would not help. For this system would then constitute a non-predictable system from the point of view of mechanics. Only if we assume that *for every* mechanical system, there exists one which is not only superior to it but, at the same time, so subtle in its operations as to be undetectable, only then would the finite doctrine of determinism be valid.

But the infinite series of predictors in whose existence or physical constructability we would have to believe would be very different from the one described in section 4. It could not be convergent—

for in this case, all predictors coming after a certain predictor P_n would differ very little from P_n and would not therefore be sufficiently dissimilar to be undetectable by P_n. In other words, the series could not converge towards some ideal predictor P_∞, for otherwise we would have to assume that all the predictors of such a series from a certain P_n on belong to one 'society.' (Moreover, even the existence of P_∞ would not help.)

Our last considerations apply only to certain classical systems of mechanics, and are therefore of a less general character than our other arguments. They are intended to show that the attempt to rescue determinism leads to assumptions which, although not logically impossible, are not only highly implausible, but incompatible with certain classical systems of physics which means that classical mechanics is inconsistent with the finite determinist doctrine that for every specified prediction task, it is possible, from the point of view of the physical system under consideration, to construct an adequate predictor (see section 3). And they show that, if there are classical mechanical predictors (and otherwise predictions do not exist at all in a physical sense, from the point of view of classical mechanics), then there must exist unpredictable predictors—predictors which are so sensitive that they would be strongly disturbed by every attempt, of a classical mechanical predictor, to predict them.

I do not claim that an analogous theorem must hold for every other (classical) system of physics, although it appears to me very probable that this is indeed the case for every system which is sufficiently complete to contain a theory which can analyse the functioning of the predictor in question. One might think, for example, that in a classical system which includes a theory of light, one could construct, for every detector P_n of light signals, a sender P_{n+1} which sends signals of a wave-length which cannot be detected by P_n, so that P_{n+1} can observe P_n without P_n detecting this fact. But this, I believe, is connected with the fact that such a classical system of physics fails to give a satisfactory account of the interaction between matter and radiation, and therefore of permanent records which retain the results of optical measurements, such as photographic plates. This, it appears, can only be done by a non-classical theory such as quantum mechanics; but to a theory of this kind our argument appears to be applicable, for reasons analogous to those indicated above.

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(Part II will be published in the next number.)

AN OUTLINE OF GENERAL SYSTEM THEORY

1 *Parallel Evolution in Science*

As we survey the evolution of modern science, we find the remarkable phenomenon that similar general conceptions and viewpoints have evolved independently in the various branches of science, and to begin with these may be indicated as follows : in the past centuries, science tried to explain phenomena by reducing them to an interplay of elementary units which could be investigated independently of each other. In contemporary modern science, we find in all fields conceptions of what is rather vaguely termed ' wholeness.'

It was the aim of classical physics eventually to resolve all natural phenomena into a play of elementary units, the characteristics of which remain unaltered whether they are investigated in isolation or in a complex. The expression of this conception is the ideal of the Laplacean spirit, which resolves the world into an aimless play of atoms, governed by the laws of nature. This conception was not changed but rather strengthened when deterministic laws were replaced by statistical laws in Boltzmann's derivation of the second principle of thermodynamics. Physical laws appeared to be essentially ' laws of disorder,' a statistical result of unordered and fortuitous events. In contrast, the basic problems in modern physics are problems of organisation. Problems of this kind present themselves in atomic physics, in structural chemistry, in crystallography, and so forth. In microphysics, it becomes impossible to resolve phenomena into local events, as is shown by the Heisenberg relation and in quantum mechanics.

Corresponding to the procedure in physics, the attempt has been made in biology to resolve the phenomena of life into parts and processes which could be investigated in isolation. This procedure is essentially the same in the various branches of biology. The organism is considered to be an aggregate of cells as elementary life-units, its activities are resolved into functions of isolated organs and finally physico-chemical processes, its behaviour into reflexes, the material substratum of heredity into genes, acting independently of each other, phylogenetic evolution into single fortuitous mutations, and so on. As opposed to the analytical, summative and machine theoretical viewpoints, organismic conceptions[1] have evolved in all branches of modern biology which assert the necessity of investigating not only parts but also relations of organisation resulting from a dynamic interaction and manifesting themselves by the difference in behaviour of parts in isolation and in the whole organism.

The development in medicine follows a similar pattern.[2] Virchow's programme of ' cellular pathology,' claiming to resolve disease into functional disturbances of cells, is to be supplemented by the consideration of the organism-as-a-whole, as it appears clearly in such fields as theory of human constitutions, endocrinology, physical medicine and psychotherapy.

Again we find the same trend in psychology. Classical association psychology tried to resolve mental phenomena into elementary units, sensations and the like, psychological atoms, as it were. *Gestalt* psychology has demonstrated the existence and primacy of psychological entities, which are not a simple summation of elementary units, and are governed by dynamical laws.

Corresponding developments are found in the social sciences. In classical economic doctrine, society was considered as a sum of human individuals as social atoms. At present there is a tendency to consider a society, an economy, or a nation, as a whole which is superordinated to its parts. This conception is at the basis of all the various forms of collectivism, the consequences of which are often disastrous for the individual and, in the history of our times, profoundly influence our lives.[3] Civilisations appear, if not as superorganisms, as was maintained by Spengler, at least as superindividual units or systems, as expressed in Toynbee's conception of history.

In philosophy, the same general trend is manifest in systems so radically different as Nicolai Hartmann's theory of categories, the doctrine of emergent evolution, Whitehead's ' organic mechanism,' and dialectic materialism ; all these are systems which are diametrically opposed in their scientific, metaphysical and social backgrounds, but agree in maintaining that principles of dynamic wholeness are basic in the modern conception of the world.

Thus, similar fundamental conceptions appear in all branches of science, irrespective of whether inanimate things, living organisms, or social phenomena are the objects of study. This correspondence is the more striking because these developments are mutually independent, largely unaware of each other, and based upon totally different facts and contradicting philosophies. They open new perspectives in science and life, but also involve serious danger.

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[1] L. von Bertalanffy, *Das biologische Weltbild*, Band I. Die Stellung des Lebens in Natur und Wissenschaft, Bern, 1949

[2] L. von Bertalanffy, *Biologie und Medizin*, Wien, 1946

[3] F. A. Hayek, *The Road to Serfdom*, Chicago, 1944

Thus it appears that there is a general change in the scientific attitude and conceptions, and the question arises : what is the origin of these correspondences ?

2 *Isomorphic Laws in Science*

Not only are general aspects and viewpoints alike in different fields of science ; we find also formally identical or isomorphic laws in completely different fields. This is a well-known fact in physics where the same differential equations apply, for example, to the flow of liquids, of heat, and of electric currents in a wire. But it appears that the significance of this fact, and the possibilities it opens in fields outside physics, have hardly been considered.

For example, the exponential law or law of compound interest applies, with a negative exponent, to the decay of radium, the monomolecular reaction, the killing of bacteria by light or disinfectants, the loss of body substance in a starving animal, and to the decrease of a population where the death rate is higher than the birth rate. Similarly, with a positive exponent, this law applies to the individual growth of certain micro-organisms, the unlimited Malthusian growth of bacterial, animal, or human populations, the growth curve of human knowledge (as measured by the number of pages devoted to scientific discoveries in a textbook on the history of science), and the number of publications on *Drosophila*.[1] The entities concerned—atoms, molecules, bacteria, animals, human beings, or books—are widely different, and so are the causal mechanisms involved. Nevertheless, the mathematical law is the same. Another equation, the logistic law of Verhulst, is, in physical chemistry, the equation of autocatalytic reaction, and in biology, it describes certain cases of organic growth. It was first stated in demography to describe the growth of human populations in a limited space of living. It governs also the advancement of technical inventions, such as the growth of the railway system in the United States during the last century, or of the number of wireless sets in operation. What is known in national economy as Pareto's law[1] of the distribution of income within a nation, represents, in biology, the law of allometric growth, describing the relative increase of organs, chemical compounds, or physiological activities with respect to body size. Volterra[2] has developed a population dynamics, comparable to mechanical dynamics, working with homologous concepts such as demographic energy and potential, life action, etc., and leading to a principle of minimum vital action, corresponding to the principle of minimum action in mechanics. Actually, principles of minimum action appear in widely different fields besides mechanics, for example, in physical chemistry as the principle of Le Chatelier and in electrodynamics as Lenz' rule. Again, the principle of relaxation oscillations governs the neon lamp, but also important phenomena in nerve physiology and certain phenomena of biocoenoses or organic communities.

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[1] A. H. Hersh, 'Drosophila and the Course of Research,' *Ohio J. of Science*, 1942, 42, 198-200

The same is true for phenomena where the general principles can be described in ordinary language though they cannot be formulated in mathematical terms. For instance, there are hardly processes more unlike phenomenologically and in their intrinsic mechanisms, than the formation of a whole animal out of a divided sea-urchin or newt germ, the re-establishment of normal function in the central nervous system after removal or injury to some of its parts, and *gestalt* perception in psychology. Nevertheless, the principles governing these different phenomena show striking similarities.

Again we ask : what is the origin of these isomorphisms ?

There are three obvious reasons. The first is in the trivial fact that while it is easy to write down any complicated differential equation yet even innocent-looking expressions may be hard to solve, or give, at least, cumbersome solutions. The number of simple differential equations which are available and which will be preferably applied to describe natural phenomena is limited. So it is no wonder that laws identical in structure will appear in intrinsically different fields. A similar consideration holds for statements formulated not in mathematical but in ordinary language : the number of intellectual schemes available is rather restricted, and they will be applied in quite different realms.

However, these laws and schemes would be of little help if the world (i.e. the totality of observable events) was not such that they

[1] W. Pareto, *Cours de l'Économie Politique*, 1897

[2] V. Volterra, *Lécons sur la Théorie Mathématique de la Lutte pour la Vie*, Paris, 1921

could be applied to it. We can imagine a chaotic world or a world which is too complicated to allow the application of the relatively simple schemes which we are able to construct with our limited intellect. Fortunately, the actual world is not of this sort, and does allow the application of our intellectual constructions.

But there is yet a third reason for the isomorphism of natural laws and this is most important for our present purpose. Laws of the kind considered are characterised by the fact that they hold generally for certain classes of complexes or systems, irrespective of the special kind of entities involved. For instance, the exponential law states that, given a complex of a number of entities, a constant percentage of these elements decay or multiply per unit time. Therefore this law will apply to the pounds in a banking account as well as to radium atoms, molecules, bacteria, or individuals in a population. The logistic law says that the increase, originally exponential, is limited by some restricting conditions. Thus in autocatalytic reactions, a compound formed catalyses its own formation ; but since the number of molecules is finite in a closed reaction vessel, the reaction must stop when all molecules are transformed, and must therefore approach a limiting value. A population increases exponentially with the increasing number of individuals, but if space and food are limited, the amount of food available per individual decreases ; therefore the increase in number cannot be unlimited, but must approach a steady state defined as the maximum population compatible with resources available. Railway lines which already exist in a country lead to the intensification of traffic and industry which, in turn, make necessary a denser railway network, till a state of saturation is eventually reached ; thus, railways behave like autocatalysers accelerating their own increase, and their growth follows the autocatalytic curve. The parabolic law is an expression for competition within a system, each element taking its share according to its capacity as expressed by a specific constant. Therefore the law is of the same form whether it applies to the competition of individuals in an economic system, following Pareto's law, or to organs competing within an organism for nutritive material and showing allometric growth.

There exist therefore *general system laws* which apply to any system of a certain type, irrespective of the particular properties of the system or the elements involved. We may say also that there is a structural correspondence or logical homology of systems in which the entities concerned are of a wholly different nature. This is the reason why we find isomorphic laws in different fields.

The need for a general superstructure of science, developing principles and models that are common to different fields, has often been emphasised in recent years, for instance, by the Cybernetics group of N. Wiener, by the General Semantics of Count Korzybski,[1] in the claim for 'Scientific Generalists' as recently advanced by Bode and others[2] and in many other publications. But a clear statement of the problem and a systematic elaboration has apparently never been made.

3. *General System Theory*

Such considerations lead us to postulate a *new basic scientific discipline* which we call *General System Theory*.[3] It is a logico-mathematical field, the subject matter of which is the formulation and deduction of those principles which are valid for 'systems' in general. There are principles which apply to systems in general, whatever the nature of their component elements or the relations or 'forces' between them. The fact that all sciences mentioned above are concerned with systems, leads to a formal correspondence or logical homology in their general principles, and even in their special laws if the conditions of the systems correspond in the phenomena under consideration.

General System Theory is a logico-mathematical discipline, which is in itself purely formal, but is applicable to all sciences concerned with systems. Its position is similar to that, for example, of probability theory, which is in itself a formal mathematical doctrine but which can be applied to very different fields, such as thermodynamics, biological and medical experimentation, genetics, life insurance statistics, etc.

The significance of the General System Theory may be characterised in different ways.

So far, exact science, meaning a mathematical hypothetico-deductive system, has been almost identical with theoretical physics, and the only systematic scientific laws that have been acknowledged universally and without restriction have been the laws of physics and chemistry. To be sure, there are rudiments of systems of laws, i.e. hypothetico-deductive systems, also in other realms such as national economy, demography, and certain fields of biology. For example, references are often made to biological or economical ' equilibria.'[1] But it remains somewhat obscure what the concept of ' equilibrium ' means, if applied outside the fields of physical magnitudes, and so conceptions of this and similar kinds have remained little more than loose, if ingenious, metaphors. Few attempts to state exact laws in non-physical fields have gained universal recognition ; they lack the consistency of the system of physics, and their methodological background remains obscure.

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[1] A. Korzybski, *Science and Sanity*, 2nd ed. New York, 1941

[2] H. Bode, *et al.*, 'The Education of a Scientific Generalist,' *Science*, 1949, **109**, 553

[3] L. von Bertalanffy, 'Zu einer allgemeinen Systemlehre,' *Blätter f. dtsche Philos.*, 1945, **18**, No. 3/4. (Not known if published.) 'Zu einer allgemeinen Systemlehre,' *Biologia Generalis*, 1949, **19**, 114–129.

In consequence of the predominant development of the physical sciences, it was thought that, in order to state exact laws for any field, and to render it an exact science, it had to be reduced to physics and chemistry. This is, of course, the methodological principle of the so-called mechanistic view.

Quite apart from the question whether the mechanistic principle is justified in the last resort and for some remote future, it appears, however, that it does not in fact work in wide fields of science. For example, we can isolate processes occurring in the living organism and describe them in the terms and laws of physico-chemistry. This is done, with enormous success, in modern biophysics and biochemistry. But when it comes to the properly ' vital ' features, it is found that they are essentially problems of organisation, orderliness, and regulation, resulting from the interaction of an enormous number of highly complicated physico-chemical events. To grasp in detail the physico-chemical organisation of even the simplest cell is far beyond our capacity. So we come to the conclusion that it is not possible to state exact laws for the basic biological phenomena, such as self-regulation in metabolism, growth, morphogenesis, behaviour, etc., because they are much too complicated to allow a thorough understanding and an analysis of all the processes involved. This is, in fact, the common opinion in present biology. The same applies, of course, even more to sociological phenomena, because of their even higher complexity and the impossibility of resolving them into physico-chemical events.

[1] H. Dotterweich, *Das biologische Gleichgewicht und seine Bedeutung für die Hauptprobleme der Biologie*, Jena, 1940

J. Dumontier, *Equilibre Physique, Equilibre Biologique, Equilibre Economique*, Paris, 1949.

Thus it seems necessary to expand our conceptual schemes if we wish to deal with these complex realms, and to make it possible for them to be included in the exact sciences ; to establish systems of exact laws also in those fields where an application of the laws of physics or chemistry is not sufficient or even possible. Even physical concepts need to, and in fact do undergo expansion and far-reaching modifications when applied to new realms, as we shall see when considering the recent extension of kinetics and thermodynamics to open systems.

As opposed to the mechanistic conception, we are led to a different view. The task of science is to state laws for the different strata of reality. Even in physics, quantum statistics, molecular statistics, and macrophysical laws represent different strata. Similarly, we may apply statistical values and laws on any level, if this gives results consistent with experience and within a theoretical system. If you cannot run after each molecule and describe the state of a gas in a Laplacean formula, take, with Boltzmann, a statistical law describing the average result of the behaviour of a great many individual molecules. If you cannot follow the enormous number of processes in intermediary metabolism, use average values such as total metabolism quotients of anabolism and catabolism, representing the outcome of all these processes, and you may be able to state exact laws for phenomena such as metabolism, growth, and morphogenesis of the organism as a whole, without the hopeless undertaking to press all individual physico-chemical processes into a gigantic formula. You cannot resolve the individuals within a biocoenosis or a social unit into cells and finally into physico-chemical processes. Very well, take the individuals as units, and eventually you will get a system of laws which is not physics but is of the same form as exact physical science, that is a mathematical hypothetico-deductive system.

However, to apply the procedure consistently, it seems necessary to establish the principles which apply to those entities which are called ' systems,' and of which physical systems are only a subclass. Thus, we are led again to the conception of a General System Theory, as a doctrine which is generalised with respect to physics. ' Kinetics ' and ' dynamics ' have been, as yet, branches of physics concerned with entities such as molecules, energy, and the like. We ask for a generalised kinetics and dynamics, where the entities concerned can be interpreted as any entities that present themselves in different fields.

General System Theory is not a mere catalogue of well-known differential equations and their solutions. On the contrary, the general system conception raises new and well-defined problems which do not appear in physics, because they are not met with in its usual problems, but which are of basic importance in non-physical fields. Just because the phenomena concerned are not dealt with in ordinary physics, these problems have often appeared as metaphysical or vitalistic. It will be an important task to generalise physical principles such as those of minimum action, of Le Chatelier, or the conditions of the existence of stationary and periodic solutions and of steady states, etc., in such a way that they apply to systems in general. Problems and concepts such as progressive mechanisation, centralisation, individuality, leading part, competition, etc., are unfamiliar to the physicist, but they are basic in the biological and sociological realms, and require exact treatment.

Moreover, General System Theory should be an important regulative device in science. The existence of laws of similar structure in different fields enables the use of systems which are simpler or better known as models for more complicated and less manageable ones. Therefore General System Theory should be, methodologically, an important means of controlling and instigating the transfer of principles from one field to another, and it will no longer be necessary to duplicate or triplicate the discovery of the same principles in different fields isolated from each other. At the same time, by formulating exact criteria, General System Theory will guard against superficial analogies which are useless in science and harmful in their practical consequences.

The central position of the concept of wholeness in biology, psychology, sociology and other sciences is generally acknowledged. What is meant by this concept is indicated by expressions such as ' system,' '*gestalt*,' ' organism,' ' interaction,' ' the whole is more than the sum of its parts ' and the like. However, these concepts have often been misused, and they are of a vague and somewhat mystical character. The exact scientist therefore is inclined to look at these conceptions with justified mistrust. Thus it seems necessary to formulate these conceptions in an exact language. General System Theory is a new scientific doctrine of ' wholeness '—a notion which has been hitherto considered vague, muddled and metaphysical.

Considered from the viewpoint of philosophy, General System Theory is to replace that field which is known as ' theory of categories'[1] by an exact system of logico-mathematical laws. Those general notions, which as yet have been formulated only in common language, will acquire, by the General System Theory, that unambiguous and exact expression which is possible only in mathematical language.

4 *The System Concept*

In the following, we shall outline a few general system principles and characteristics in order to illustrate the programme indicated above.

A system can be defined as a complex of interacting elements $P_1, p_2 \ldots p_n$. Interaction means that the elements stand in a certain relation, R, so that their behaviour in R is different from their behaviour in another relation, R′. On the other hand, if the behaviour in R and R′ is not different, there is no interaction, and the elements behave independently with respect to the relations R and R′.

If we denote the measure of some quantitative aspect of the elements p by $Q_1, Q_2 \ldots Q_n$, its variation may be defined by a system of simultaneous differential equations which, for a finite number of elements and in the simplest case, will be of the form :

$$\left.\begin{aligned}\frac{dQ_1}{dt} &= f_1(Q_1, Q_2, \ldots Q_n)\\ \frac{dQ_2}{dt} &= f_2(Q_1, Q_2, \ldots Q_n)\\ \frac{dQ_n}{dt} &= f_n(Q_1, Q_2, \ldots Q_n)\end{aligned}\right\} \quad . \quad . \quad . \quad (1)$$

Equations (1) can be considered as expressing a general principle of kinetics. Systems of equations of this kind are found in many fields. In *Simultankinetik*, as developed by Skrabal,[2] it is the general expression of the law of mass action. The same system has been used by Lotka [3] in a rather broad sense, and especially with respect to demographic problems. The equations for biocoenotic systems, as given by Volterra, Lotka, D'Ancona, Gause and others, are special cases of the general system of equations (1). So are the equations used by Spiegelman[1] for kinetics of cellular processes and the theory of competition within an organism. Werner[2] has stated a similar though somewhat more general system (considering the system as continuous, and using therefore partial differential equations with respect to x, y, z, and t) as the basic law of pharmacodynamic action from which the various laws of drug action can be derived by introducing the corresponding special conditions.

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[1] N. Hartmann, 'Neue Wege der Ontologie,' in *Systematische Philosophie*, ed. N. Hartmann, Stuttgart and Berlin, 1942.

[2] A. Skrabal, 'Von den Simultanreaktionen,' *Ber. dtsch. chem. Ges.* (A), 1944, 77, 1-12 ; 'Die Kettenreaktionen anders gesehen,' *Mh. Chemie*, 1949, 80, 21-57

[3] A. J. Lotka, *Elements of Physical Biology*, Baltimore, 1925

Although nothing has been said about the nature of the elements or about the functions $f_1, f_2 \ldots f_n$, i.e. about the relations or interactions within the system, certain general principles can be deduced. We may use our equations (1) in order to show the structural isomorphism in different fields and levels of reality; or, in other words, in order to demonstrate the possibility of a General System Theory whose fields of application are to be found in various sciences. The parameters and variables will have, of course, a different meaning in each case of application.

We may first consider the simplest case, viz, that the system consists of elements of only one kind. In this case our system of equations is reduced to a single equation:

$$\frac{dQ}{dt} = f(Q), \quad \ldots \quad (2)$$

which may be developed into a Taylor series:

$$\frac{dQ}{dt} = a_1Q + a_{11}Q^2 + \ldots \quad \ldots \quad (3)$$

This series does not contain an absolute term in the case in which there is no 'spontaneous generation' of elements. Then dQ/dt must disappear, for $Q = 0$, which is possible only if the absolute term is equal to zero.

The simplest possibility is realised when we retain only the first term of the series:

$$\frac{dQ}{dt} = a_1Q, \quad \ldots \quad (4)$$

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[1] S. Spiegelman, 'Physiological Competition as a Regulatory Mechanism in Morphogenesis,' *Quart. Rev. Biol.*, 1945, **20**, 121

[2] G. Werner, 'Beitrag zur mathematischen Behandlung pharmakologischer Fragen.' *Sitz. Akad. Wiss. Wien, Math.-naturw. Kl.*, 1947, Abt. IIa, **156**, 457–467

This signifies that the growth of the system is directly proportional to the number of elements present. Depending on whether the constant a_1 is positive or negative, the growth of the system is positive or negative, and the system increases or decreases. The solution is :

$$Q = Q_0 e^{a_1 t}, \qquad . \quad . \quad . \quad . \quad (5)$$

Q_0 signifying the number of elements at $t = 0$. This is the *exponential law* which, as already said (p. 136), is found in many fields.

Going back to equation (3) and retaining two terms, we have :

$$\frac{dQ}{dt} = a_1 Q + a_{11} Q^2. \qquad . \quad . \quad . \quad . \quad (6)$$

A solution of this equation is :

$$Q = \frac{a_1 C e^{a_1 t}}{1 - a_{11} C e^{a_1 t}} \qquad . \quad . \quad . \quad . \quad (7)$$

Keeping the second term has an important consequence. The simple exponential (5) shows an infinite increase ; taking into account, however, the second term, we obtain, according to (7), a curve which is sigmoid and attains a limiting value. This curve is the so-called *logistic* curve, and is also of wide application (p. 136).

If the system consists of elements of two or more different varieties, its evolution can follow different lines. This may be illustrated by the simplest case, i.e. a system consisting of two kinds of elements, Q_1 and Q_2 :

$$\left.\begin{aligned} \frac{dQ_1}{dt} &= f_1(Q_1, Q_2) \\ \frac{dQ_2}{dt} &= f_2(Q_1, Q_2) \end{aligned}\right\} \qquad . \quad . \quad . \quad (8)$$

Provided that the functions can be developed into Taylor series, the solution is of the form :

$$\left.\begin{aligned} Q_1 &= Q_1^* - G_{11} e^{\lambda_1 t} - G_{12} e^{\lambda_2 t} - G_{111} e^{2\lambda_1 t} - \ldots \\ Q_2 &= Q_2^* - G_{21} e^{\lambda_1 t} - G_{22} e^{\lambda_2 t} - G_{211} e^{2\lambda_1 t} - \ldots \end{aligned}\right\} . \quad (9)$$

where Q_1^*, Q_2^* are the stationary values of Q_1, Q_2, obtained by setting $f_1 = f_2 = 0$; the G's are integration constants ; and the λ's are roots of the characteristic equation :

$$\begin{vmatrix} a_{11} - \lambda & a_{12} \\ a_{21} & a_{22} - \lambda \end{vmatrix} = 0,$$

or developed :

$$(a_{11} - \lambda)\,(a_{22} - \lambda) - a_{12}a_{21} = 0,$$

$$\lambda^2 - \lambda C + D = 0,$$

$$\lambda = \frac{C}{2} \pm \sqrt{-D + \left(\frac{C}{2}\right)^2},$$

with

$$C = a_{11} + a_{22}\;;\;\; D = a_{11}a_{22} - a_{12}a_{21}.$$

In the case :

$$C < 0,\; D > 0,\; E = C^2 - 4D > 0,$$

both solutions of the characteristic equation are negative. Therefore the system will approach a stable stationary state where $Q_1 = Q_1^*$, $Q_2 = Q_2^*$, since $e^{-\infty} = 0$, and therefore the second and following terms continually decrease.

In the case :

$$C = 0,\; D > 0,\; E < 0,$$

both solutions are imaginary. Therefore the system contains periodic terms, and there will be oscillations or cycles around the stationary values.

Finally in the case :

$$C > 0,\; D < O,\; E > 0,$$

both solutions are positive, and there is no stationary state.

Similar considerations apply to systems with n components.

5 *Wholeness, Sum, Mechanisation, Centralisation*

The concepts just indicated have often been considered to describe characteristics only of living beings, or even to be a proof of vitalism. In actual fact they are formal properties of systems.

I Let us assume again that the equations (1) can be developed into Taylor series :

$$\frac{dQ_1}{dt} = a_{11}Q_1 + a_{12}Q_2 + \ldots a_{1n}Q_n + a_{111}Q^2 + \ldots \quad (10)$$

We see that any change in some quantity, Q_1, is a function of the quantities of all elements, Q_1 to Q_n. On the other hand, a change in a certain Q_i causes a change in all other elements and in the total system. The system therefore behaves as a *whole*, the changes in every element depending on all the others.

II Let the coefficients of the variables Q_j $(j \neq i)$ now become zero. The system of equations degenerates into :

$$\frac{dQ_i}{dt} = a_{i1}Q_i + a_{i11}Q_i^2 + \ldots \qquad (11)$$

This means that a change in each element depends only on that element itself. Each element can therefore be considered independent of the others. The variation of the total complex is the (physical) sum of the variations of its elements. We may call such behaviour *physical summativity* or *independence.*

We may define summativity by saying that a complex can be built up, step by step, by putting together the first separate elements ; conversely, the characteristics of the complex can be analysed completely into those of the separate elements. This is true for those complexes which we may call ' heaps,' such as a heap of bricks or odds and ends, or for mechanical forces, acting according to the parallelogram of forces. It does not apply to those systems which were called *Gestalten* in German. Take the most simple example ; three electrical conductors have a certain charge which can be measured in each conductor separately. But if they are connected by wires, the charge in each conductor depends on the total constellation, and is different from its charge when insulated.

Though this is trivial from the viewpoint of physics, it is still necessary to emphasise the non-summative character of physical and biological systems because the methodological attidude has been, and is yet to a large extent, determined by the mechanistic programme.[1] In Lord Russell's latest book [2], we find a rather astonishing rejection of the ' concept of organism.' This concept states, according to Russell, that the laws governing the behaviour of the parts can be stated only by considering the place of the parts in the whole. Russell rejects this view. He uses the example of an eye, the function of which as a light receptor can be understood perfectly well if the eye is isolated and if only the internal physico-chemical reactions, and the incoming stimuli and outgoing nerve impulses, are taken into account. ' Scientific progress has been made by analysis and artificial isolation. . . . It is therefore in any case prudent to adopt the mechanistic view as a working hypothesis, to be abandoned only where there is clear evidence against it. As regards biological phenomena, such evidence, so far, is entirely absent.' It is true that the principles of summativity are applicable to the living organism to a certain extent. The beat of a heart, the twitch of a nerve-muscle preparation, the action potentials in a nerve are much the same if studied in isolation or within the organism as a whole. This applies to those phenomena we shall define later as occurring in highly 'mechanised' partial systems. But Russell's statement is profoundly untrue with respect exactly to the basic and primary biological phenomena. If you take any realm of biological phenomena, whether embryonic development, metabolism, growth, activity of the nervous system, biocoenoses, etc., you will always find that the behaviour of an element is different within the system from what it is in isolation. You cannot sum up the behaviour of the whole from the isolated parts, and you have to take into account the relations between the various subordinated systems and the systems which are super-ordinated to them in order to understand the behaviour of the parts. Analysis and artificial isolation are useful, but in no way sufficient, methods of biological experimentation and theory.

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[1] L. von Bertalanffy, *Das biologische Wellbied,* I

[2] B. Russell, *Human Knowledge, Its Scope and Limits,* London, 1948

III There is a further case which appears to be unusual in physical systems but is very common and basic in biological, psychological and sociological systems. This case is that in which the interactions between the elements decrease with time. In terms of our basic model equation (1), this means that the coefficients of the Q_i are not constant, but decrease with time. The simplest case will be :

$$\lim_{t \to \infty} a_{ij} = 0 \qquad \qquad (12)$$

In this case the system passes from a state of wholeness to a state of independence of the elements. The primary state is that of a unitary system which splits up gradually into independent causal chains. We may call this *progressive segregation.*

As a rule, the organisation of physical wholes, such as atoms, molecules, or crystals, results from the union of pre-existing elements. In contrast, the organisation of biological wholes is built up by the differentiation of an original whole which segregates into parts. An example is determination in embryonic development, when the germ passes from a state of equipotentiality to a state where it behaves like a mosaic or sum of regions which develop independently into definite organs. The same is true in the development and evolution of the nervous system and of behaviour starting with actions of the whole body or of large regions and passing to the establishment of definite centres and localised reflex arcs, and for many other biological phenomena.

The reason for the predominance of segregation in living nature seems to be that segregation into subordinate partial systems implies an increase of complexity in the system. Such transition towards higher order presupposes a supply of energy, and energy is delivered continuously into the system only if the latter is an open system, taking energy from its environment. We shall come back to this question later on (p. 158).

The principle of progressive segregation has peculiar implications. Progress is possible in any biological, psychological, or sociological organisation, only by passing from a state of undifferentiated wholeness to differentiation of the parts. This implies, however, that the parts become fixed with respect to a certain action. Therefore progressive segregation also means progressive mechanisation.

Progressive mechanisation, however, implies loss of ability to be regulated. As long as a system is a unitary whole, a disturbance will be followed by the attainment of a new stationary state, due to the interactions within the system. The system will ' regulate ' itself. If, however, the system is split up into independent causal chains, regulability disappears. The partial processes will go on irrespective of each other. This is the behaviour we find, for example, in embryonic development, determination going hand in hand with decrease of regulability.

In this duality of segregation, implying progressive differentiation and mechanisation, lies the tragic character of every evolution. Progress is possible only by subdivision of an initially unitary action into actions of specialised parts. This, however, means at the same time loss in other respects. The more parts are specialised in a certain direction, the more they are irreplaceable, and loss of parts leads to the breakdown of the whole system. To speak in Aristotelian language, every evolution, by unfolding some potentiality, nips in the bud many other possibilities. We may find this in embryonic development as well as in phylogenetic specialisation, or in specialisation in science or daily life[1].

Behaviour as a whole and summative behaviour, unitary and elementalistic conceptions, are usually regarded as being antitheses. But it is frequently found that there is no opposition between them, but a gradual transition from behaviour as a whole to summative behaviour. This insight is important for the clarification of many biological concepts. As can easily be shown many controversies about concepts like those of the gene or the nerve centre have arisen because only the limiting cases, namely, complete interaction or independence of causal trains, have been taken into account, and not the intermediates which have a wide application.[1]

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[1] L. von Bertalanffy, *Das biologische Weltbild*, I, pp. 50 ff.

IV Connected with this is yet another principle. Suppose that the coefficients of one element, p_s, are large in all equations while the coefficients of the other elements are considerably smaller or even equal to zero. In this case the system may look like this :

$$\left.\begin{aligned} \frac{dQ_1}{dt} &= a_{11}Q_1 + \ldots a_{1s}Q_s + \ldots \\ \frac{dQ_s}{dt} &= a_{s1}Q_s + \ldots \\ \frac{dQ_n}{dt} &= a_{ns}Q_s + \ldots a_{n1}Q_n + \ldots \end{aligned}\right\} \quad . \quad . \quad (13)$$

if we write the linear members only, for simplicity.

We may call the element p_s a *leading part*, or say that the system is *centred* around p_s. If the coefficients a_{is} of p_s in some or all equations are large while the coefficients in the equation of p_s itself are small, a small change in p_s will cause a considerable change in the total system. p_s may be then called a *trigger*. A small change in p_s will be ' amplified ' in the total system. From the energetic viewpoint, in this case we do not find ' conservation causality ' (*Erhaltungskausalität*) where the principle ' *causa aequat effectum* ' holds, but ' instigation causality ' (*Anstosskausalität*),[2] an energetically insignificant change in p_s causing a considerable change in the total system.

The principle of centralisation is especially important in the biological realm. Progressive segregation is often connected with progressive centralisation, the expression of which is the time-dependent evolution of a leading part, i.e. a combination of the schemes (12) and (13). At the same time, the principle of progressive centralisation is that of progressive individualisation. An ' individual ' can be defined as a centralised system. Strictly speaking this is, in the biological realm, a limiting case, only approached ontogenetically and phylogenetically, the organism growing through progressive centralisation more and more unified and 'more indivisible.'[1]

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[1] L. von Bertalanffy, *Das biologische Weltbild*, I, pp. 73 ff., 111 ff.

[2] A. Mittasch, *Von der Chemie zur Philosophie*. Ausgewählte Schriften und Vorträge, Ulm, 1948

V In many systems the components are themselves systems of a next lower order. This means that any Q_i is itself a system of elements, R_i, where R_i may be defined by a system of equations like (1). This is called *hierarchical order.*

6 *Competition*

Our system of equations also indicates the competition between the parts.

The simplest possible form is, again, that all coefficients $(a_{j \neq i}) = 0$ i.e. that the increase in each element depends only on this element itself. Then we have, for two elements :

$$\left.\begin{aligned} \frac{dQ_1}{dt} &= a_1Q_1 \\ \frac{dQ_2}{dt} &= a_2Q_2 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (14)$$

or

$$\left.\begin{aligned} Q_1 &= c_1e^{a_1t} \\ Q_2 &= c_2e^{a_2t} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (15)$$

Eliminating time, we obtain :

$$t = \frac{\ln Q_1 - \ln c_1}{a_1} = \frac{\ln Q_2 - \ln c_2}{a_2}, \quad . \quad . \quad (16)$$

and

$$Q_1 = BQ_2{}^A, \quad . \quad . \quad . \quad . \quad (17)$$

with $A = \frac{a_1}{b_1}$, $B = \frac{c_1}{c_2{}^A}$.

This is the equation known in biology as the *allometric equation.* In this discussion, the simplest form of growth of the parts, viz, the exponential, has been assumed (14 and 15). The allometric relation holds, however, also for somewhat more complicated cases, such as growth according to the parabola, the logistic, the Gompertz function, either strictly or as an approximation.[2]

[1] L. von Bertalanffy, *Das biologische Weltbild*, I, pp. 55 ff.

[2] H. Lumer, 'The Consequences of Sigmoid Growth Curves for Relative Growth Functions,' *Growth*, 1937, 1

The allometric equation applies to a wide range of morphological, biochemical, physiological and phylogenetic data. It means that a certain characteristic, Q_1, can be expressed as a power function of another characteristic, Q_2. Take, for instance, morphogenesis. Then the length or weight of a certain organ, Q_1, is, in general, an allometric function of the size of another organ, or of the total length or weight of the organism in question, Q_2. The meaning of this becomes clear if we write equations (14) in a slightly different form :

$$\frac{dQ_1}{dt} \cdot \frac{1}{Q_1} : \frac{dQ_2}{dt} \cdot \frac{1}{Q_2} = A, \qquad (18)$$

or

$$\frac{dQ_1}{dt} = A \cdot \frac{Q_1}{Q_2} \cdot \frac{dQ_2}{dt} \qquad (19)$$

Equation (18) states that the relative growth rates (i.e. increase calculated as a percentage of the actual size) of the parts under consideration, Q_1 and Q_2, stand in a constant proportion throughout the life, or during a life cycle for which the allometric equation holds. This rather astonishing relation (because of the immense complexity of growth processes it would seem, at first, unlikely that the growth of parts is governed by an algebraic equation of such simplicity) is explained by equation (19). According to this equation, it can be interpreted as a result of a process of distribution. Take Q_2 for the whole organism ; then equation (19) states that the organ Q_1 takes, from the increase resulting from the metabolism of the total organism $\left(\frac{dQ_2}{dt}\right)$, a share which is proportional to its actual proportion to the latter $\left(\frac{Q_1}{Q_2}\right)$. A is a partition coefficient indicating the capacity to seize its share of the organ. If $a_1 > a_2$, i.e. if the growth intensity of Q_1 is greater than that of Q_2, then $A = \frac{a_1}{a_2} > 1$; the organ captures more than other parts ; it grows therefore more rapidly than these or with positive allometry. Conversely, if $a_1 < a_2$, it is $A > 1$, the organ grows more slowly, or shows negative allometry. Similarly, the allometric equation applies to biochemical changes in the organism, and to physiological functions. For instance, basal metabolism increases, in wide groups of animals, with $A = \frac{2}{3}$ with respect to body weight if growing animals of the same species, or animals of related species, are compared; this means that basal metabolism is, in general, a surface function of body weight. In certain cases, such as insect larvae and snails, $A = 1$, i.e. basal metabolism is proportional to weight itself.

In sociology, the expression in question is *Pareto's law*, [1] which we have already mentioned (p. 137). Pareto's law (1897) is $Q_1 = BQ_2^A$, with Q_1 = number of individuals gaining a certain income, Q_2 = amount of the income, and B and A constants. The explanation is similar to that given above, substituting for 'increase of the total organism' the national income, and for 'distribution constant' the economic abilities of the individuals concerned.

The situation becomes more complex if interactions between the parts of the system are assumed, i.e. if $a_{j \neq i} \neq 0$. Then we come to those systems of equations which have been stated by Volterra [2] for competition among species, and, correspondingly, by Spiegelman [3] for competition within an organism. Since these cases are fully discussed in the literature,[4] we shall not enter into a detailed discussion. Only one or two points of general interest may be mentioned.

Thus, it is an interesting consequence that, in Volterra's equations, competition of two species for the same resources is, in some way, more fatal than a predator-prey relation, i.e. partial annihilation of one species by the other. For competition eventually leads to the extermination of the species with the smaller growth capacity; a predator-prey relation only leads to periodic oscillation of the numbers of the species concerned around a mean value. These relations have been stated for biocoenotic systems, but it may well be that they have also sociological implications.

Another point of philosophical interest should be mentioned. If we are speaking of 'systems,' we mean 'wholes' or 'unities.' Then it seems paradoxical at first that, with respect to a whole, the concept of competition between its parts is introduced. In fact, however, these apparently contradictory statements both belong to the essentials of systems. Every whole is based upon the competition of its elements, and presupposes the 'struggle between parts' (Roux). The latter is a general principle of organisation in simple physico-chemical systems as well as in organisms and social units, and it is, in the last resort, an expression of the *coincidentia oppositorum* that reality presents.[1]

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[1] W. Pareto, *Cours de l'Économic Politique*

[2] V. Volterra, *Leçons sur la Théorie Mathématique de la Lutte pour la Vie*

[3] S. Spiegelman, 'Physiological Competitions as a Regulatory Mechanism in Morphogenesis'

[4] L. von Bertalanffy, *Theoretische Biologie*, Band II, Stoffwechsel, Wachstum, Berlin, 1942; 2nd edition, Bern (in press)

U. D'Ancona, *La Lotta per l'Esistenza*, Torino, 1942; German Translation, *Der Kampf ums Dasein*, Abh. z. exakten Biologie, ed. by L. von Bertalanffy, Heft 1, Berlin, 1939

7 *Finality*

As we have seen, systems of equations of the type considered may have three different kinds of solution. The system in question may asymptotically attain a stable stationary state with increasing time ; or it may never attain a stable stationary state ; or there may be periodic oscillations. In the case in which the system approaches a stationary state, its variation can be expressed not only in terms of the actual conditions but also in terms of the distance from the stationary state. If Q_i^* are the solutions for the stationary state, new variables :

$$Q_i = Q_i^* - Q_i'$$

can be introduced so that

$$\frac{dQ_i}{dt} = f\,(Q_1^* - Q_1')\,(Q_2^* - Q_2')\;.\;.\;.\;(Q_n^* - Q_n') \qquad (20)$$

Thus it seems as if the system would 'aim toward' an equilibrium to be reached only in the future. This is especially obvious when we consider the integral of such equations : it may always be written in a form containing the final values. It looks therefore as if the actual process were determined by final values to be reached in infinite time only. But this final value is, of course, simply a limit which we obtain by equating the derivatives to zero so that it drops out. The 'teleological' final value formula is only a transformation of the differential equation indicating the actual conditions ; the 'directiveness' of the process toward a final state is not different from causality, but another expression of it.

Although this consideration is obvious from the mathematical and physical standpoints, the point in question has been often misunderstood. Philosophers have asked how it is possible that a state A depends upon a state B which does not yet exist. Similarly, biologists, under the influence of the tendency to make analogies between finalistic events and human actions which are determined by the foresight of a goal, have rejected expressions containing final values or, conversely, have seen in this finality a proof of vitalism.

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[1] L. von Bertalanffy, *Das biologische Weltbild*, I, pp. 60 ff.

Thus a number of concepts follow from the definition of systems which, like non-summativity, wholeness, centralisation, individuality, finality, have often been considered as anthropomorphic, vitalistic or metaphysical, but which are, in fact, consequences of the formal characters and certain conditions of systems.

8 *Closed and Open Systems*

We now come to a consideration which leads to important problems and discoveries in physics, biology and other fields.

It is the basic characteristic of every organic system that it maintains itself in a state of perpetual change of its components. This we find at all levels of biological organisation. In the cell there is a perpetual destruction of its building materials through which it endures as a whole. Recent research, the investigations with isotope-tracers, have shown that this exchange of building materials goes on at a rate much higher than was formerly supposed. In the multicellular organism, cells are dying and are replaced by new ones, but it maintains itself as a whole. In the biocoenosis and the species, individuals die and others are born. Thus every organic system appears stationary if considered from a certain point of view. But what seems to be a persistent entity on a certain level, is maintained, in fact, by a perpetual change, building up and breaking down of systems of the next lower order : of chemical compounds in the cell, of cells in the multicellular organism, of individuals in ecological systems.

The characteristic state of the living organism is that of an open system. We call a system closed if no materials enter or leave it. It is open if there is inflow and outflow, and therefore change of the component materials.

So far, physics and physical chemistry have been almost exclusively concerned with closed systems. However, the consideration of organisms and other living systems makes necessary an extension and generalisation of theory. The kinetics and thermodynamics of open systems have been developed in recent years. The present writer has advanced since 1932 the conception of the organism as an open system and has stated general kinetic principles and their biological implications.[1, 2] Similar investigations have been made by Burton,[3] Dehlinger and Wertz,[4] Skrabal,[5] Reiner and Spiegelman,[6] Denbigh,[7] and others. The thermodynamic of open systems has been developed by Prigogine,[8] and Prigogine and Wiame.[9] Since a survey of the theory of open systems has been given recently[10] we mention here only a few points of general and philosophical significance.

The consideration of open systems is more general in comparison with that of closed systems; for it is always possible to come from open to closed systems by equating the transport terms to zero, but not *vice versa.* In physics, the theory of open systems leads to basically new, and partly revolutionary, consequences and principles. In biology it accounts, first, for many characteristics of living systems which have appeared to be in contradiction with the laws of physics, and have been considered hitherto as vitalistic features. Secondly, the consideration of organisms as open systems yields quantitative laws of basic biological phenomena, such as metabolism and growth, form development, excitation, etc.

In the case in which the variations in time disappear, systems become stationary. Closed systems thus attain a time-independent state of equilibrium where the composition remains constant. In fact, closed systems *must* eventually reach a state of equilibrium, according to the second law of thermodynamics. Open systems *may*, provided certain conditions are given, attain a stationary state. Then

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[1] L. von Bertalanffy, *Theoretische Biologie*, Band II

[2] L. von Bertalanffy, 'Der Organismus als physikalisches System betrachtet,' *Naturwissenschaften*, 1940, **28**, 521-531

[3] A. C. Burton. 'The Properties of the Steady State as Compared to those of Equilibrium as Shown in Characteristic Biological Behavior,' *J. cell. comp. Physiol.*, 1939, **14**, 327

[4] U. Dehlinger and E. Wertz, 'Biologische Grundfragen in physikalischer Betrachtung,' *Naturwissenschaften*, 1942, **30**, 250

[5] A. Skrabal, 'Das Reaktionsschema der Waldenschen Umkehrung. III,' *Oesterr. Chemiker Ztg.*, 1947, **48**, 158-163

[6] J. M. Reiner and S. Spiegelman, 'The Energetics of Transient and Steady States,' *J. phys. Chem.*, 1945, **49**, 81

[7] K. G. Denbigh, *et al.* 'The Kinetics of Open Reaction Systems,' *Trans. Faraday Soc.*, 1948, **44**, 479-494

[8] I. Prigogine, *Étude Thermodynamique des phénomènes irréversibles*, Paris, 1947

[9] I. Prigogine and J. M. Wiame, 'Biologie et Thermodynamique des phénomènes irréversibles,' *Experientia* (Basel), 1946, **2**, 450-451

[10] L. von Bertalanffy, 'The Theory of Open Systems in Physics and Biology,' *Science*, 1950, **3**, 23-29

the system appears also to be constant, though this constancy is maintained in a continuous change, inflow and outflow of materials. This is called a *steady* state. Since there is no equivalent for this expression in German, the term *Fliessgleichgewicht* was introduced by the author. Steady states can be realised in certain physico-chemical arrangements and are, in fact, widely used in technological chemistry. Living systems are the most important examples of open systems and steady states.

9 *Equifinality*

A profound difference between most inanimate and living systems can be expressed by the concept of *equifinality*. In most physical systems the final state is determined by the initial conditions. Take, for instance, the motion in a planetary system where the positions at a time t are determined by those of a time t_0, or a chemical equilibrium where the final concentrations depend on the initial ones. If either the initial conditions or the process is modified, the final state is changed.

Vital phenomena show a different behaviour. Here, to a wide extent, the final state may be reached from different initial conditions and in different ways. Such behaviour we call equifinal. Thus, for instance, the same final result, namely a typical larva, is achieved by a complete normal germ of the sea urchin, by a half germ after experimental separation of the cells, by two germs after fusion, or after translocations of the cells. It is well-known that it was just this experiment which was considered, by Driesch, the main proof of vitalism. According to Driesch, such behaviour is inexplicable in physico-chemical terms. For a physico-chemical system cannot achieve the same performance, in this case the production of a whole organism, if divided or injured. This extraordinary performance can be accomplished only by the action of a vitalistic factor, entelechy, essentially different from physico-chemical forces and governing the processes in foresight of the goal to be reached. It is therefore a question of basic importance whether equifinality is a proof of vitalism. The answer is that it is not.

Analysis shows that closed systems cannot behave equifinally. This is the reason why equifinality is found in inanimate nature only in exceptional cases. However, in open systems, which are exchanging materials with the environment, in so far as they attain a steady state, the latter is independent of the initial conditions, or is equifinal. Thus, in an open kinetic system, irrespective of the content in the beginning or any other time, the steady state values will always be the same because they are determined only by the constants of reaction and of inflow and outflow. Steady state systems show equifinality, in sharp contrast to closed systems in equilibrium where the final state depends on the components given at the beginning of the process.

Equifinality can be formulated quantitatively in certain biological cases. Thus growth is equifinal : the same final size which is characteristic for the species can be reached from different initial sizes (e.g. in litters of different numbers of individuals) or after a temporary suppression of growth (e.g. by a diet insufficient in quantity or in vitamins). According to the quantitative theory advanced by the author[1] growth can be considered the result of a counteraction of the anabolism and catabolism of building materials. Since in the most common type of growth, anabolism is a function of surface, catabolism of body mass, the surface-volume ratio is shifted in disfavour of surface with increasing size. Therefore a balance between anabolism and catabolism will eventually be reached which is independent of the initial size and depends only on the species-specific ratio of the metabolic constants. It is therefore equifinal.

Equifinality is at the basis of organic regulations. We find it everywhere where biological events are determined by the dynamic interactions of parts ; it becomes progressively restricted and finally impossible when the originally unitary system segregates into separate causal chains determined by fixed structures, that is to say, with progressive segregation. There are two general restrictions of regulation. The first is the incompleteness of the open-system-character of the organism. For instance, the growth regulations mentioned will not be possible if insufficient diet has caused lasting irreversible disturbances, for example, in the ossification of bones. The second limitation of regulation lies in the hierarchical order, namely, in the progressive segregation of the organism into subordinate systems which gain a certain independence of each other. The extreme case is a tumour which behaves as if it were an independent organism, and thus destroys the whole of which it is a part.

Thus the investigation of open systems leads to a conclusion which is very remarkable for the philosophy of science. The equifinal form of directiveness which is so characteristic for biological phenomena that it has been considered the vitalistic essence of life is, in fact, a necessary consequence of the steady state in organisms.

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[1] L. von Bertalanffy, 'Das organische Wachstum und seine Gesetzmässigkeiten,' *Experientia* (Basel), 1948, 4, 255-269

L. von Bertalanffy, 'Problems of Organic Growth,' *Nature*, London, 1949, **163**, 156-158

10 *Types of Finality*

Since it is not possible to enter here into a detailed discussion of the problem of finality, we are merely enumerating the different types found in experience. Thus we can distinguish :

I. Static teleology or fitness, meaning that an arrangement seems to be useful for a certain ' purpose.' Thus a fur coat is fit to keep the body warm, and so are hairs, feathers, or layers of fat in animals. Thorns may protect plants against grazing cattle, or imitative colourations and mimicries may be advantageous to protect animals against enemies.

II. Dynamic teleology, meaning a directiveness of processes. Here different phenomena can be distinguished which are often confused :

(i) Direction of events towards a final state which can be expressed as if the present behaviour were dependent on that final state. Every system which attains a time-independent condition behaves in this way.

(ii) Directiveness based upon structure, meaning that an arrangement of structures leads the process in such a way that a certain result is achieved. This is true, of course, of the function of man-made machines yielding products or performances as desired. In living nature we find a structural order of processes that in its complication widely surpasses all man-made machines. Such order is found from the function of macroscopic organs, such as the eye as a sort of camera, or the heart as a pump, to the microscopic cell structures responsible for metabolism, secretion, excitability, heredity and so forth. Whilst man-made machines work in such a way as to yield certain products and performances, for example, fabrication of airplanes or moving a railway train, the order of process in living systems is such as to maintain the system itself. An important part of these processes is represented by homeostasis (Canon), i.e. those processes through which the material and energetical situation of the organism is maintained constant. Examples are the mechanisms of thermoregulation, of maintenance of osmotic pressure, of pH, of salt concentration, the regulation of posture and so forth. These regulations are governed, in a wide extent, by feed-back mechanisms. Feed-back means that from the output of a machine a certain amount is monitored back, as 'information,' to the input so as to regulate the latter and thus to stabilise or direct the action of the machine. Mechanisms of this kind are well known in technology, as, for instance, the governor of the steam-engine, self-steering missiles and other 'servomechanisms.' Feed-back mechanisms appear to be responsible for a large part of the organic regulations and phenomena of homeostasis, as recently emphasised by Cybernetics.[1]

(iii) There is, however, yet another basis for organic regulations. This is equifinality, i.e. the fact that the same final state can be reached from different initial conditions and in different ways. This is found to be the case in open systems, in so far as they attain a steady state. It appears that equifinality is responsible for the primary regulability of organic systems, i.e. for all those regulations which cannot be based upon predetermined structures or mechanisms but, on the contrary, exclude such mechanisms and were regarded therefore as arguments for vitalism.

(iv) Finally, there is true finality or purposiveness, meaning that the actual behaviour is determined by the foresight of the goal. This is the original Aristotelian concept. It presupposes that the future goal is already present in thought, and directs the present action. True purposiveness is characteristic of human behaviour, and it is connected with the evolution of the symbolism of language and concepts.[2]

The confusion of these different types of finality is one of the factors responsible for the confusion occurring in epistemology and theoretical biology. In the field of man-made things, fitness (I) and teleological working of machines (II, ii) are, of course, due to a planning intelligence (II, iv). Fitness in organic structures (I) can probably be explained by the causal play of random mutations and natural selection. This explanation is, however, much less plausible for the origin of the very complicated organic mechanisms and feed-back systems (II, ii). Vitalism is essentially the attempt to explain organic directiveness (II, ii and iii) by means of intelligence in foresight of the goal (II, iv). This leads, methodologically, beyond the limits of natural science, and is empirically unjustified, since we have, even in the most astonishing phenomena of regulation or instinct, no justification for, but most definite reasons against, the assumption that, for example, an embryo or an insect is endowed with superhuman intelligence. A most important part of those phenomena which have been advanced as ' proofs of vitalism,' such as equifinality and anamorphosis (see below), are consequences of the characteristic state of the organism as an open system, and thus accessible to scientific interpretation and theory.

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[1] L. K. Frank, *et al.* 'Teleological Mechanisms,' *Ann. N.Y. Acad. Sci.*, 1948, 50
N. Wiener, *Cybernetics*, New York and Paris, 1948

[2] L. von Bertalanffy, 'Das Weltbild der Biologie,' in *Weltbild und Menschenbild*, III. Internat. Hochschulwochen. des Oesterr. College, Salzburg, 1948, pp. 251-274

11 *Catamorphosis and Anamorphosis*

' According to definition, the second law of thermodynamics applies only to closed systems, it does not define the steady state.' [1] The expansion of thermodynamics to include open systems was elaborated by Prigogine [2] and has led to most important results of which we shall discuss only a few of far-reaching significance.

The direction of happenings in closed systems is towards states of maximum entropy since, according to the second law, entropy must increase in all irreversible processes. But this is not true in open systems. In an open system, and especially in a living organism, there is not only a production of entropy due to irreversible processes, but the organism ' feeds,' to use an expression of Schrödinger's, ' from negative entropy.' It imports complex organic molecules, uses their energy, and renders back the simpler end-products to the environment. Therefore the total change of entropy can be negative as well as positive. Though the second law is not violated, more strictly speaking, though it holds for the system plus its environment, it does not hold for the open system itself. Entropy may decrease in such systems, and their steady states are not defined by maximum entropy but, as demonstrated by Prigogine, by minimum entropy production.

As is well known, the significance of the second law can be expressed also in another way. It states that the general tendency of events is towards a state of maximum disorder. According to the second law, higher forms of energy such as mechanical energy, light, electricity and so forth, are continually and irreversibly degraded to undirected heat movement. Heat gradients, in turn, are gradually levelled down, and so the Universe approaches entropy death as its irrevocable fate when all energy is converted into heat of low temperature, and the world process comes to a stop. There may be exceptions to the second law in microphysical dimensions since in the interior of stars, under very high temperatures, higher atoms are built up from simpler ones, especially helium from hydrogen. These processes are the source of sun radiation and are the basis of the hydrogen bomb. But on the macrophysical level, the second law seems essentially to command a transition toward maximum disorder and degradation.

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[1] L. von Bertalanffy, *Theoretische Biologie*, Band II

[2] I. Prigogine, *Étude Thermodynamique des phénomènes irréversibles*

But here a striking contrast between inanimate and animate nature seems to exist. According to the second law, physical events are directed towards a levelling down of differences and states of maximum disorder. In organic development and evolution, a transition towards states of higher order and differentiation seems to occur. It has often been assumed therefore that a tendency toward increasing complication is a primary characteristic of the living, in contrast to inanimate nature. This was called anamorphosis by Woltereck.[1]

These problems gain new aspects if we pass from closed systems, which alone are taken into account by classical thermodynamics, to open systems. Entropy may decrease in the evolution of such systems ; in other words, such systems may spontaneously develop towards greater heterogeneity and complexity. Probably it is just this thermodynamical characteristic of organisms as open systems which is at the basis of the apparent contrast of catamorphosis in inorganic, and anamorphosis in living nature. This is obviously so in the transition towards higher complexity in organic development which is possible only at the expense of energies yielded by oxidation and other energy-yielding processes. The transition toward higher complexity is connected with the splitting up of a primary unitary system into partial systems. This seems to be the reason why we find what we have called progressive segregation only in the organic realm. With respect to evolution, these considerations show that the supposed violation of physical laws does not exist, or more strictly speaking, that it disappears by the extension of physical theory.

It is outside our present task to discuss the application of the theory of open systems to special problems in physics and biology.[2, 3] But it may be emphasised that just the peculiar and supposedly vitalistic characteristics of vital phenomena take on a new appearance in the theory of open systems. Equifinality, which was brought forward by Driesch as the ' first proof' of vitalism, is a consequence of steady state conditions. Similarly, self-regulation in metabolism was considered as explicable only by a governing entelechy ; but its general characteristics follow from the laws of steady states. Anamorphosis conflicts with classical thermodynamics, but is in accordance with thermodynamics of open systems. Self-multiplication of biological elementary units, such as genes and chromosomes, was offered by Driesch as a ' second proof' of vitalism. If a hypothesis, advanced by the author[1] should prove to be correct, it would also be a consequence of the fact that these units are metabolising systems. I think therefore that we do not go far astray if we suppose that the principles of open systems are near the very root of the central biological problems.

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[1] R. Woltereck, *Ontologie des Lebendigen*, Stuttgart, 1939

[2] L. von Bertalanffy, 'The Theory of Open Systems in Physics and Biology.' *Theoretische Biologie*, Band II

[3] I. Prigogine, *Étude Thermodynamique des phénomènes irréversibles*

12 *The Unity of Science*

We may summarise the main results of this presentation as follows :

(*i*) The analysis of general system principles shows that many concepts which have often been considered as anthropomorphic, metaphysical, or vitalistic, are accessible to exact formulation. They are consequences of the definition of systems or of certain system conditions.

(*ii*) Such investigation is a useful prerequisite with respect to concrete problems in science. In particular, it leads to the elucidation of problems which, in the usual schematisms and pigeonholes of the specialised fields, are not envisaged. Thus system theory should prove an important means in the process of developing new branches of knowledge into exact science, i.e. into systems of mathematical laws.

(*iii*) This investigation is equally important to Philosophy of Science, major problems of which gain new and often surprising aspects.

(*iv*) The fact that certain principles apply to systems in general, irrespective of the nature of the systems and of the entities concerned, explains that corresponding conceptions and laws appear independently in different fields of science, causing the remarkable parallelism in their modern development. Thus, concepts such as wholeness and sum, mechanisation, centralisation, hierarchical order, stationary and steady states, equifinality, etc., are found in different fields of natural sciences, as well as in psychology and sociology.

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[1] L. von Bertalanffy. ' Bemerkungen zum Modell der biologischen Elementareinheiten,' *Naturwissenschaften*, 1944, **32**, 26–32

These considerations have a definite bearing on the question of the Unity of Science. The current opinion has been well represented by Carnap.[1] As he states, Unity of Science is granted by the fact that all statements in science can ultimately be expressed in physical language, i.e. in the form of statements that attach quantitative values to definite positions in a space-time system of co-ordinates. In this sense, all seemingly non-physical *concepts*, for instance specifically biological notions such as 'species,' 'organism,' 'fertilisation,' and so forth, are defined by means of certain perceptible criteria, i.e. qualitative determinations capable of being physicalised. The physical language is therefore the universal language of science. The question whether biological *laws* can be reduced to physical ones, i.e. whether the natural laws sufficient to explain all inorganic phenomena are also sufficient to explain biological phenomena, is left open by Carnap, though with preference given to an answer in the affirmative.

From our point of view Unity of Science wins a much more concrete and, at the same time, profounder aspect. We too leave open the question of the 'ultimate reduction' of the laws of biology (and the other non-physical realms) to physics, i.e. the question whether a hypothetico-deductive system embracing all sciences from physics to biology and sociology may ever be established. But we are certainly able to establish scientific laws for the different levels or strata of reality. And here we find, speaking in the 'formal mode' (Carnap), a correspondence or isomorphy of laws and conceptual schemes in different fields, granting the Unity of Science. Speaking in 'material' language, this means that the world (i.e. the total of observable phenomena) shows a structural uniformity, manifesting itself by isomorphic traces of order in its different levels or realms.

Reality, in the modern conception, appears as a tremendous hierarchical order of organised entities, leading, in a superposition of many levels, from physical and chemical to biological and sociological systems. Unity of Science it granted, not by a utopian reduction of all sciences to physics and chemistry, but by the structural uniformities of the different levels of reality.

Especially the gap between natural and social sciences or, to use the more expressive German terms, of *Natur- und Geisteswissenschaften* is widely diminished, not in the sense of a reduction of the latter to biological conceptions but in the sense of structural similarities. This is the cause of the appearance of corresponding general viewpoints and notions in both fields, and may eventually lead to the establishment of a system of laws in the latter.

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[1] R. Carnap, *The Unity of Science*, London, 1934

The mechanistic world-view found its ideal in the Laplacean spirit, i.e. in the conception that all phenomena are ultimately aggregates of fortuitous actions of elementary physical units. Theoretically, this conception did not lead to exact sciences outside the field of physics, i.e. to laws of the higher levels of reality, the biological, psychological and sociological. Practically, its consequences have been fatal for our civilisation. The attitude that considers physical phenomena as the sole standard-measure of reality, has lead to the mechanisation of mankind and to the devaluation of higher values. The unregulated domination of physical technology finally ushered the world into the catastrophical crises of our time. After having overthrown the mechanistic view, we are careful not to slide into ' biologism,' that is, into considering mental, sociological and cultural phenomena from a merely biological standpoint. As physicalism considered the living organism as a strange combination of physico-chemical events or machines, biologism considers man as a curious zoological species, human society as a bee-hive or a stud-farm. Biologism has, theoretically, not proved its theoretical merits, and has proved fatal in its practical consequences. The organismic conception does not mean a unilateral dominance of biological conceptions. When emphasising general structural isomorphies of different levels, it asserts, at the same time, their autonomy and possession of specific laws.

We believe that the future elaboration of General System Theory will prove to be a major step towards the unification of science. It may be destined, in the science of the future, to play a role similar to that of Aristotelian logic in the science of antiquity. The Greek conception of the world was static, things being considered to be a mirroring of eternal archetypes or ideas. Therefore classification was the central problem in science, the fundamental organon of which is the definition of subordination and superordination of concepts. In modern science, dynamic interaction appears to be the central problem in all fields of reality. Its general principles are to be defined by System Theory.

Ludwig von Bertalanffy

The Unitary Principle in Physics and Biology, L. L. Whyte, Cresset Press, London, 1949. Pp. ix + 182. 12s. 6d.

In this book we find an attempt to offer a new foundation establishing unity and order in knowledge so far as science is concerned. In the introduction we are shown the respects in which physics and biology are thought to be lacking, and it is suggested that there is need for some comprehensive principle under which both branches are subsumed. We are trying to find the process of which all processes are variants. Since process is the subject matter and causality is a component of process, we attempt to discover a ' single principle representing the observable, causally necessary relation structure of all one-way processes.' The study of one-way processes is more general than that of cyclic processes, for the latter are special cases of one-way processes.

Mr Whyte suggests that this new foundation is to be based on describing process from a new standpoint. In so far as a complete description of a process demands that that process be isolated, we must examine the criteria under which a process shall be said to be isolated. We are told that ' those processes are isolable for the purpose of scientific representation which display internal causal continuity,' therefore ' the most fundamental characteristic of any science is the type of cause-effect relation which it assumes, and the class of systems which it thereby selects as isolable.'

The usually accepted basis is one of conservation (' the cause equals the effect,' which does not seem to be a very happy way of putting it). The new method involves a more general case based on the ' inequality of cause and effect.' This can be expressed in a principle of symmetry : ' The symmetry of causes must be repeated in their effects, and any asymmetry of effects must therefore be present in their causes. The degree of symmetry of an isolable process cannot decrease ; new asymmetries cannot arise in an isolable process.'

We may ask ourselves what is meant, in this context, by symmetry and asymmetry. Mr Whyte has previously told us about certain relations which are logically asymmetrical, therefore we assume that the ' degree of symmetry ' refers to the number and character of those relations within the system which are logically symmetrical, and similarly for the notion of asymmetry. We must not fall into the mistake of supposing that asymmetry or symmetry is being used itself as a relation. I do not think the author means this. He does not mean that Cause A is asymmetrical to Cause B, but that there is a relation (or set of relations) between A and B, and these relations are all asymmetrical in the sense we find defined in textbooks on logic. This interpretation is borne out by the following quotation : ' Expressed in more general terms this means that the procedure of nature must operate on existing distinctions, it cannot produce new distinctions where none existed before.'

To the principle of symmetry is added a second characteristic of causality which is hypothetical : asymmetry can decrease in isolable processes. Therefore we are offered ' as an axiom of a potentially universal science the Unitary Principle : Asymmetry tends to disappear, and this tendency is realised in isolable processes.'

Before continuing with an account of what Mr Whyte has to say about this principle in physics and biology, we should first ask what we or he hopes this principle is going to do. Is it an axiom from which statements in physics and biology are derivable ? Or is it a methodological postulate which, when adopted, will enable us to make new observations, invent new generalisations and high level theoretical statements which are more comprehensive than those in use today ? In other words, do we use this principle to determine when processes are isolable and then describe such a process, generalise about it, and so on ? Or do we attempt to work out consequences (we shall need some further axioms, that is obvious) and see if we arrive at testable generalisations ? Let us see what the author thinks about the principle :

> ' The unitary principle defines a universal method of selecting causally simple process within the complexity of nature.'
>
> ' It may thus be regarded equally as a proposed law of nature or as a rule for selecting isolable processes.'
>
> ' Thus the unitary principle represents the process both in nature as a whole and in that part of nature which we call the mind.'
>
> ' When the unitary principle is applied to any special type of process, it leads at once to the identification of clear issues for isolated examination.'

From these quotations and many more, it seems that the principle is believed to be both an axiom in a new theory and a methodological postulate—or metatheoretical statement of a new theory. This situation is elliptical if the two new theories are one and the same theory, and from the second quotation Mr Whyte appears to think they are. The theory he would call Unitary Theory. Perhaps it is evidence of the limits of knowledge that fundamental statements involving causality tend to be both postulates and axioms in the same sphere. Surely the problem is resolvable by refraining from bringing causal relations as such into science. We could say that if event A is before in time to event B, and stands in a relation R (which is at least spatial) to event B then the event C is after in time to the total event—(A in relation R and before in time to event B). This method of formulating a causal situation avoids the discussion of whether A or B is the cause of C, and avoids the notion of cause itself.

If the principle is not functioning as an axiom and postulate in the same theory, and if we consider it to be an axiom we would expect Mr Whyte to show us later in the book that some physical and biological generalisations are *derivable* from it. Unfortunately nowhere in the book is this concisely and clearly done. It may be a fault of exposition, or it may not have been attempted.

On the other hand, if the principle is functioning as a postulate then we shall expect to be shown how certain concepts in physics and biology can be expressed on the basis of the isolation of systems having decreasing asymmetry. This is extensively done in both sciences, and it is extremely interesting to see the sort of concept that is involved in the interpretation of certain usual concepts in the two sciences.

Before discussing this aspect in more detail, it is worth suggesting to the author that the provision of detailed examples and bibliography for many of his statements will greatly aid the reader. As it is, he forces his readers into three camps : Those who are sympathetic to his aim will endeavour to seek examples which support his statements. Those who are unsympathetic will take his statements at their face value and will easily find intuitive counter examples. The rest, probably the majority, will sink in a sea of unitary terminology which in the absence of references appears practically meaningless.

In the principle, the word asymmetry refers to spatial relations between 'nuclei, atoms, molecules and more extensive patterns.' However, it must not be confused with asymmetry in enantiomorphic forms, which in the book is called chirality. The significance of the principle is, therefore, that it is an attempt to carry physics a stage further in a reduction to geometry, for instability in a system implies structural asymmetry among its molecules, whereas stability implies symmetry among its molecules. This correlation between structure and stability is particularly interesting in biology where the initial differentiation of form followed by the associated stability and corresponding preservation of form for a time stretch of an organism (e.g. in an adult) offers scope for this sort of analysis. Unfortunately, in his initial exposition of biological and mental processes Mr Whyte seems more concerned to show that there are many instances of logically asymmetrical relations between parts of organisms in biological processes and between mental events in mental processes. This tends to cloud the concept of asymmetry of *structural* relations and we are left wondering whether, after all, the unitary principle is being expounded in terms of *general* asymmetrical relations. Later, however, we are returned to structural asymmetry and we find it used in connection with mental processes by making the latter functions of spatial neural patterns. This does not make pages 54-57 seem to be less mystical.

After discussing certain common concepts e.g. polarisation ; parts and wholes ; thresholds ; facilitation, etc., in the light of unitary theory (interpreting them, as it is called), we find the question of biological organisation discussed in two parts. First, there is an examination of the various properties of differentiated systems of living protein. It is here that we have to be careful to analyse what is being presented. Are we being told something we already know in new words ? Or are many of the statements derivable generalisations from the principle as an axiom ? The author suggests they are consequences of the principle (p. 112 on living protein). If this is the case he has been unsuccessful in one instance in demonstrating to a reader the form of derivation. I suggest that they look like interpretations in a complicated language of some aspects of facts ; that they are hypothetical in that they say more than we know, and that therefore, references are essential. A few examples of such interpretations will serve to show the difficulties besetting the reader. We are told that the unitary characterisation of a living protein is : ' a linear structure with side groups providing an adequately stabilised and extensive continuity of cyclically variable resultant field so that extended normalising processes are propagated along appropriate channels ' ; or that in the case of semi-permeable membranes : ' every disturbance not only upsets the local polarisation perpendicular to the membrane, it also destroys the continuity along the membrane, sending concertina-like shivers in all directions. This shivering of the membrane facilitates the movement through it of chemical structures of appropriate symmetry type. . . .'

In fairness I must say that we are told what a normalising process is in terms of unitary theory, what polarisation is, and what facilitating involves in terms of structure, but when these processes are incorporated into such complex notions as I have given, without a few references to facts to help intuition, the most sympathetic reader feels rather deserted !

Secondly and lastly, there is a consideration of how these processes are combined in the complete organism so that life is maintained. The discussion is centred round an initial provisional formula for an animal organism : ' An animal organism is a continuous normalising process stabilised by hereditary units and an outer boundary.'

The correctness of this formula is to be judged by deducing from it all the general properties of animal organisms as tendencies possessed by that class of systems and also by the successful application of the formula to clarify the distinguishing characteristics of special groups of organisms. Once again the exposition is not clear enough to follow the stages in the deduction of general properties. Any apparent deductions reveal themselves as very high level hypotheses especially when seen in summary on p. 125. For example, we are told that :

> ‘ The synthesis of each specific protein is evoked as a component of the normalising of a field of corresponding specific pattern, and results in a structure which facilitates the repetition of the process by which it was formed, either by extending its specific pattern or by undergoing cycles of polarisation and depolarisation.’

If we take some statements as suggestions on an intuitive basis we find many interesting comments. For example we find that under unitary theory self-regulation appears to be a natural conclusion of differentiative development, for the latter leads to an organised system with the property of physiological self-regulation. If this can be shown to be the case it is a most interesting correlation. One can think of differentiative development leading to systems which do not show self-regulative properties, e.g. nails, hairs, etc., but then, these may not be *organised* systems and we are back wondering whether it is not another case of ‘ it all depends upon what you mean by. . . .’

Some of the most interesting parts of the book are to be found in sections 43, 44, and 45, in which Mr Whyte attempts to distinguish between green plants, primitive organisms and animal organisms, and thus to arrive at a general definition of an organism. In describing the ways in which green plants differ from animals we are reminded of the differences of basic synthesis. It is pointed out that the green plant is able to impose chirality upon simple carbon and nitrogen compounds. Recent tracer research has shown that following injection of bicarbonate this compound can be synthesised in the animal organism into carbohydrate. This means that in some conditions chirality can be imposed by animals as well as plants. What Mr Whyte does not emphasise (unless it be by his use of the words ‘ *basic* synthesis ’), is that since plants always do this they are less exacting in their food requirements than animals ; we do not find most green plants requiring ‘ necessary ’ complex organic compounds such as necessary amino-acids. Is it, perhaps, a characteristic of this less exactingness that plants possess a mechanism for imposing chirality on the simple substances ?

It is hoped that readers will be stimulated by this review to adopt a sympathetic approach to Mr Whyte’s book. The Unitary Principle as a methodological postulate may turn out to be one of the most important suggestions in methodology for some years. As an axiom from which we can deduce, one feels less confident, but perhaps the author will supply us with a clearer formulation of his theory so that we can see its position in his system. Also perhaps the author will take pity on the less informed and supply them with examples, references and perhaps even an index.

One thing is certain from reading the book, and that is that much informed thought has gone into it. We should therefore first assume that such objections as we may find may be the fault of our own understanding and perhaps the exposition, so that in writing a critical review we should aim at understanding rather than the exercise of literary sadism.

R. F. J. WITHERS

Sir Isaac Newton : *Theological Manuscripts.* Selected and Edited with an Introduction by H. McLachlan. Liverpool University Press, 1950. Pp. viii + 147. 7s. 6d.

THE MSS chosen by Dr McLachlan for publication have been selected from the vast mass of Newton's unpublished papers. These papers, when they come to be fully studied, will doubtless form the basis for a complete reassessment of Newton's thought. In the meantime the publication of the MSS is an important step in this direction.

Dr McLachlan's present selection is unfortunately rather disjointed. Had he juxtaposed carefully papers on chronology, theological doctrine, church ritual, prophecy, and even alchemy, he could have shown the connection existing between these different branches of Newton's thought. Alternatively a complete edition of Newton's work on Arianism and Athanasius (in which Dr McLachlan seems mainly interested) would have provided a preliminary basis for a study of Newton's theology.

In his Introduction Dr McLachlan gives a full account of the history of the MSS since Newton's death, and discusses the origin of Newton's Unitarian views. He also gives a brief outline of Newton's theological thought, but somehow fails to reveal clearly its logical structure. The MSS themselves show that Newton held the love of God and the love of Humanity to be the two basic principles of all true religion, leaving all further doctrine open to individual interpretation. He himself believed that there is no real Scriptural authority for the doctrine of the Trinity. He was also convinced that Athanasius, in his fight against Arianism, was not only seditious but also a liar and a forger. Since these are not the views generally held by modern scholars it would have been helpful to the lay reader if Dr McLachlan had pointed out where Newton's conclusions are incorrect and where his information and his authorities are deficient.

The MS dealing with prophetic language is of considerable interest as it provides the link between Newton's prophetic and alchemical work. Dr McLachlan has, however, not attempted to deal with the relationship between Newton's theology and his science. He has omitted from his selection a passage which shows the connection existing between Newton's prophetic God and the God described in the last Query of the *Opticks.* Nevertheless Dr Mclachlan's book is of interest for showing a little-known aspect of Newton's thought, and one which Newton himself considered of equal importance with his science.

D. CASTILLEJO

BOOKS RECEIVED FOR REVIEW

Inclusion of books in this list does not preclude their being reviewed in later issues

Agnes Arber, *The Natural Philosophy of Plant Form*, Cambridge University Press, 1950, pp. xiv + 247, 25s.

G. W. Ardley, *Aquinas and Kant*, Longmans, Green & Co., London, 1950, pp. x + 256, 18s.

Max Born, *Natural Philosophy of Cause and Chance*, Oxford University Press, 1949, pp. viii + 215, 17s. 6d.

P. W. Bridgman, *Reflections of a Physicist*, Philosophical Library, Inc., New York, 1950, pp. xii + 392, $5.00.

S. J. Curtis, *A Short History of Western Philosophy in the Middle Ages*, Macdonald & Co. (Publishers) Ltd., London, 1950, pp. x + 286, 15s.

Rene J. Dubos, *Louis Pasteur, Free Lance of Science*, Little, Brown & Co., Boston, U.S.A., 1950 (Published simultaneously by McClelland & Stewart, Canada), pp. xii + 418, with illustrations, $5.00.

Marvin Farber, *Philosophical Thought in France and the United States*, University of Buffalo Publications in Philosophy, Buffalo, N.Y., 1950, pp. x + 775, $7.50.

Philipp Frank, *Modern Science and its Philosophy*, Harvard University Press (London : Geoffrey Cumberlege), 1949, pp. ix + 324, 30s.

Paul Freedman, *The Principles of Scientific Research*, Macdonald & Co. (Publishers) Ltd., London, 1949, pp. xi + 222, 15s.

Gerald Heard, *Morals since 1900*, Andrew Dakers Ltd., London, 1950, pp. 223, 12s. 6d.

C. E. M. Joad, *A Critique of Logical Positivism*, Gollancz, London, 1950, pp. 154, 10s. 6d.

Philipp Lenard, *Great Men of Science*, G. Bell & Sons Ltd., London, 1950 (Reissue), pp. xix + 389, 15s.

Henry Margenau, *The Nature of Physical Reality*, McGraw-Hill Publishing Co. Ltd., New York and London, 1950, pp. xiii + 479, $6.50 and 55s. 6d.

P. A. Schilpp—editor—*Albert Einstein : Philosopher Scientist*, The Library of Living Philosophers, Inc., Evanston, Ill., U.S.A. (London : Cambridge University Press), 1950, pp. xvi + 781, 75s.

E. W. F. Tomlin, *The Great Philosophers : The Western World*, Skeffington & Son, Ltd., London, 1950, pp. 296 (18 illustrations), 15s.

J. R. Weinberg, *Nicolaus of Autrecourt : A Study in 14th Century Thought*, Princeton University Press (London : Geoffrey Cumberlege), 1948, pp. viii + 242, 24s.